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MIND

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OF
PSYCHOLOGY AND PHILOSOPHY

EDITED BY
PROF. GILBERT RYLE
WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—COMPLEMENTARY DESCRIPTIONS

BY PETER ALEXANDER

1.—*Introduction*

I WISH to discuss a kind of theory which is gaining currency as an account of the relation between science and religion, but which is also claimed to have a wider application. Stated briefly the theory is that it is sometimes necessary to give two apparently inconsistent descriptions of a thing or situation, but that the appearance of inconsistency is removed when we see that the two descriptions are complementary and both needed for a full understanding of the thing or situation, and that it is a logical mistake either to mix the language of the two descriptions or to suppose that either will refute the other. It has been suggested that complementary descriptions are needed much more often than is usually realized and that the failure to see this leads to many of the puzzles which needlessly worry us.

I shall deal mainly with the relation between science and religion, since it is mainly in this connexion that the theory has been put forward, but I use it simply as a means of beginning an examination of the use and logic of such descriptions. I want to ask what can be meant by 'complementary descriptions', when, if ever, we accept them and, if they are appropriate anywhere, whether they are appropriate in such contexts as this.

I shall not quarrel with the implicit assumption, made by those who support the theory, that religion sets out to describe something. I suppose that it sets out to describe the relations between God and the natural world. A discussion of this peculiar use of 'description' would no doubt be fruitful; I prefer to discuss, here, the alleged complementarity of descriptions because it seems to me that there are certain general lessons

to be learnt from such a discussion. I am more interested in the logic of possible complementary descriptions than in a precise account of the function of religion. I assume throughout that one who takes religion seriously considers that, when he gives the religious description of the world, he is claiming to talk about an objective reality, at least when he uses the word 'God', and not exclusively about his own experiences and reactions.

2. *Statement of the Theory*

My interest in this was aroused by some almost incidental remarks made by Dr. D. M. MacKay at the end of his contribution to a symposium, *Mentality in Machines*, at a Joint Session of the Mind Association and the Aristotelian Society.[1] He argued that a description of human behaviour can be given either in mechanical, scientific terms or in what I shall for convenience call 'mental' terms, *i.e.* in terms of purposes, choices, decisions, and so on. Each is, or can be, an exhaustive description 'in terms of its own categories', but each is appropriate to a different context. But although each is, in this sense, exhaustive, the complete description of human behaviour requires both. To mix the terms of the two descriptions is to commit a logical error. Thus, and this was the bearing of MacKay's argument in the symposium, it is illegitimate to point to features of human behaviour, such as consciousness of purpose, with which the mechanical description cannot deal and suggest that this description is therefore incomplete. This is like the relation between science and religion. He said: "As in the parallel case of 'science and religion' the activity is not one of exhaustive *explanation*, but of complementary *description*. Each description one should expect to be exhaustive in terms of its own categories; but to apprehend the whole requires a discipline in the perception of complementarity which we have scarcely begun to acquire."

These hints were developed by MacKay in a broadcast in the series *Science and Faith*.[2] The scientist attempts to give as complete an account as possible of human behaviour from the standpoint of an *observer*, using language which presupposes that standpoint, but he does not thereby deny the validity of other accounts, presupposing the standpoint of an *actor*. You cannot dispose of the scientific account by showing that it leaves out purposes, since its presuppositions ensure from the start that

[1] Arist. Soc. Supp. Vol. XXVI, 1952, p. 86.
[2] *The Listener*, 11th September, 1952.

talk about purposes is irrelevant and inappropriate to that account. Mind is not in the physical inventory, it does not belong there, any more than an algebraical problem is in the inventory of the patches of ink used in writing it down. I quote several passages from this talk. The italics are mine.

" . . . the algebra problem is not a ghost inhabiting one of the ink patches." A man cannot find it as " something left over after making an inventory of all the ink on the page. He will find it only by a different approach *to the very same data.*" " . . . you cannot ' debunk ' one account *of a situation* merely by producing a different account that *fits the facts.* If both are *supported by the facts* and *consistent with one another* (like the ink-description and the algebra-description) then both are true and that is that ! The two, as we say, are complementary." If we are meeting a friend we can give two descriptions of what happens—" he rises to his feet wreathed in smiles and greets us heartily " and " a mass of pink protoplasm rises to a height of five feet and begins to pucker and wobble up and down noisily ". In one context the first description is appropriate, in another the second.

The basis of Christian belief, MacKay continues, is " coming to know a living Person " and " . . . all kinds of experiences, still *perfectly describable in ordinary terms* can come to *demand* interpretation as meetings with that Person ". " Now from the very nature of my research I am convinced . . . that all these experiences are mine through processes of which complementary accounts could, in principle, be given in terms of psychology and physics . . . the *significance* of what happens . . . is most appropriately described in terms of personal encounter. To substitute one of the other descriptions would do less than justice to the facts."

Recently, in a lecture at Leeds and three lectures [1] at Durham, Professor C. A. Coulson has put what I take to be the same view. He was concerned to show, like MacKay, that a scientist could, without inconsistency, be a Christian. He, too, used the idea of complementarity. Religious and scientific accounts of the world are two among several different accounts of one reality, like views from different points, to use his own analogy, of Ben Nevis. Each one can say something about everything but cannot alone give a complete account of the whole. Yet each is complete in its context. They no more conflict, because they differ, than do two different views of Ben Nevis for " . . .

[1] C. A. Coulson, *Christianity in an Age of Science*, Riddell Memorial Lectures, 1953.

different views *of the same reality* will appear different and yet both be valid" (p. 21, my italics). To describe Ben Nevis as a whole we need descriptions from all geographical points of view: to understand the world as a whole we need the scientific description, the religious description, and others besides. "There is a sort of complementarity about it." The difference between the two accounts is the difference "between the actor and spectator viewpoints of *one single situation*". One will not refute the other. We can only ask "whether the concepts that are used really do *give significance to* the experiences that provoke them" (p. 23, my italics). The religious description, Coulson insists, and here the Ben Nevis analogy admittedly breaks down, does not deal with what is left over as undescribable in scientific language but with the very same reality as the scientific description.

I do not want to argue merely over the use of the word 'complementary' nor to attack specifically these statements of the theory, for there may be more satisfactory ways of stating it. I want to try to discover whether the conception of complementarity can form the basis of a possible theory about the relation between science and religion or, indeed, any other such pairs of descriptions. I shall, therefore, list the features which these statements have in common and which seem to be essential to the theory.

(*a*) We sometimes have to give two apparently irreconcilable descriptions of a given thing or situation. They arise from a different approach to the situation and so do not really conflict but are complementary. One example is the scientific and religious descriptions of the world; another, the mechanical and mental descriptions of human behaviour.

(*b*) Both of a pair of complementary descriptions can be said to be true, or to have application.

(*c*) This allows us to say that science can give an account of everything without excluding the possibility that religion can also give an account of everything. Religion does not deal with what is 'left over' by science.

(*d*) The scientist who wants to give, also, a religious description can agree with his colleagues who do not see the need for it upon 'the facts'. That is, he need not claim access to any special or peculiar 'facts'—he has the air of having nothing up his sleeve.

3. *The Meaning and Application of 'Complementary Descriptions'*

I take complementary descriptions to be descriptions which complete one another and I believe that there is no point, in this context, in calling two descriptions 'complementary' unless

they are about the same thing or situation. I shall examine the sense of this later. There is a trivial sense in which two descriptions might be said to complement one another, which I think has no bearing here. I refer to the sense in which two items in an inventory complement one another or go to complete the whole inventory, *e.g.* two apples in an inventory of all the apples in a storehouse. I shall ignore this sense.

Now, as long as it is a *situation* that we are describing and not merely our reactions to it, there seem to be only two kinds of circumstance in which two descriptions of the same thing or situation could be given.

(*a*) The first kind is that in which one description can, in principle, deal with all the features of the thing or situation with which another description can deal, but one is more appropriate or more useful or less misleading in certain contexts. Both descriptions, that is, deal with the same features. In this case, one description is, or should be, an exact translation of the other and only one of them is *theoretically* necessary. For example, if the aims of phenomenalism were realized, either the sense-datum language or the material object language could be used and one would not be more correct or complete than the other, but only more convenient or less misleading for certain purposes. This is shown by the fact that attacks on phenomenalism often consist of challenges, which phenomenalists try to meet, to produce translations of given material object sentences. These would be more properly called ' alternative ' than ' complementary '.

(*b*) The other kind of circumstance in which two descriptions of the same thing or situation can be given is that in which the thing or situation is complex in such a way that certain of its features cannot, in principle, be dealt with by one sort of description, while certain other features cannot be dealt with by another sort of description, so that both descriptions are needed to deal with all its features and no single description can be found to replace them. Here one description can truly be said to complement the other to give the full description and they can properly be called ' complementary '. Compare with this our expressions ' complementary colours ' and ' complementary angles '. To distinguish this from another use which I shall give later, I shall refer to this as ' my definition ' of ' complementarity ', merely with the aim of avoiding the question-begging word ' normal '.

There may seem to be other circumstances, or at least one other, in which two different descriptions may be given of the same situation. It may be argued that giving one description

rather than another, applying a new name, sometimes makes us see the very same situation differently. This is not interesting, here, if it means simply 'makes us react differently to the situation'. If it does not mean this, it must mean 'makes us see something in the situation which, before, we missed' and we may take that to mean 'makes us see a situation different from the one we saw before'. If we insist on the first expression 'makes us see something in *the situation*, *i.e.* the *same* situation, which, before, we missed' this is consistent with my definition; if we use the second it is not, because then we are talking not of two descriptions of one situation but of two descriptions of different situations and no question of complementarity arises.

It is evident that one of these two things is what we must say about the relation between the scientific and religious descriptions, if we want to talk about them in these terms at all. That is, if we want the religious description to have application, to have objective reference, as I think those who take it seriously do, we must say either that they are descriptions of different features of the same situation or that they are descriptions of different situations. We want to say, that is, that certain things, physical things perhaps, are signs of God's existence and activity and we do not want to mean by this that they are merely *taken to be* signs but that they *are* signs and that something we find in them, in some sense of 'find' and 'in', shows us this. MacKay, for instance, holds that 'meeting a Person' is 'something much more than a mere metaphor'. Moreover, one who gives the religious description wants to hold that what they are signs of is an object or a Person worthy of worship in a way that no mere physical object or ordinary person is, so that the signifying function of the situation is one of its most important features from the religious point of view. I shall have more to say about this.

In general, it seems, whatever else we mean by 'complementarity' we must hold, if both descriptions are needed, that from one standpoint we see something in the situation different from what we see from another standpoint. I shall continue to call what demand the different descriptions different 'features'.

Now the words 'alternative' and 'complementary', as I am using them, are mutually exclusive, since two descriptions either are or are not translatable. For a given pair of descriptions we must decide between the two words. MacKay and Coulson do not appear to have done this and oscillate between the two conceptions. Their use of the word 'complementary' seems to be confused. It may turn out that the reconciliation of religion

and science along these lines, to obtain the advantages sought, requires both conceptions so that one inconsistency would be removed only at the cost of introducing another.

For example, MacKay and Coulson talk sometimes as if the religious and scientific descriptions spring from a different approach *to the very same data*, while the scientific description is not incomplete, which looks as if they are being put forward as alternatives. But MacKay, at other times, says that *all* kinds of experiences *demand* also a different kind of description, as if there were some features you discovered when you approached them in one way and missed when you approached them in another; and Coulson, by his Ben Nevis analogy, suggests that the religious and scientific descriptions spring from the seeing of different features of one thing from different points of view. This looks more like complementarity in my sense.

It is clear that we need not spend much time on alternative descriptions, since it is doubtful if the religious person can be satisfied with the view that the religious description could be exactly translated, without remainder, into scientific language, and *vice versa*. It is the very essence of the difficulty that this is not enough, for if the religious account says no more than the scientific account but only says it in different language, then the religious account is unnecessary, at least as description. I shall assume that this is granted.

Given, then, that the idea of complementarity implies that the two descriptions are about the same situation, in some sense, why does anyone want to hold that the religious and scientific descriptions are complementary? The main reason given, which I have mentioned but not discussed, seems to me to be an odd one. It is that if we held that the two descriptions were about different situations, this would involve holding that there were some situations with which science cannot deal and that the religious description deals with these. This is regarded as dangerous because the history of science shows the progressive extension of its boundaries, one generation thinking that science cannot deal with x and the next generation showing how it can. Thus there is a danger that science will squeeze religion out by leaving nothing for it to describe. So it is tempting to say that science and religion deal with the same things but use different concepts or different languages, that is, give *alternative* descriptions. This also has the advantage of suggesting that we all start from agreement upon 'the facts'. But when it is seen that, if we want to give the religious description, alternative descriptions are not enough, the first step is forgotten and the

idea of *complementary* descriptions is introduced, with its implication that one description says something about the situation that the other does not say. This is an attempt to get the best of both worlds. We want to say that this ' something ' refers to a feature of the situation—that is, we want to say both that the two descriptions are of the same situation and that the situation seen from one point of view is different from the situation seen from the other point of view.

I should like now to consider how the theories of the complementarity of (*a*) the religious and scientific descriptions and (*b*) the mechanical and mental descriptions, fit in with my definition. To anticipate, it looks as if, when ' complementarity ' is used in my sense, little is gained by calling these descriptions ' complementary ', while if all that is required by the supporters of the view is put into the meaning of the word, considerable violence is done to the meanings of certain other words and, moreover, the alleged advantages are not obtained.

(*a*) *The Scientific and Religious Descriptions*

If we want to say *both* that the scientific and religious descriptions are about the same situation *and* that science can, in principle, deal with every situation, *i.e.* that the religious description does not merely deal with what is left out by the scientific one, then we must, I think, define the situation as physical, in the sense of being observable in the ordinary way by all normal persons. This physical definition does not appear in my definition but is necessary here in order that all of us, including those who see no need for the religious description, may be able to agree on what ' the situation ', in any given instance, is. Otherwise the scientist could not agree, as scientist, that both descriptions described the same situation. On the other hand, as I have argued, if the religious description is to be taken seriously, we must admit that one approach to the situation shows us something in it that the other approach does not. Let me say, for convenience, that the situation is complex in such a way that some of its *features* are seen only from one point of view, while other *features* are seen only from another. This allows us to talk of the two descriptions as dealing with different features of the same situation. This leads to a classification of some of the kinds of features which are present, allegedly, in the sorts of situation under discussion. This classification is rough and not claimed to be exhaustive. The names I have used may be misleading but they will perhaps be sufficient for the present purpose. Some such analysis seems essential if we are to say all that the supporters of the theory want to say.

(1) *Physical Features:* those observable by all normal people, *e.g.* colours, shapes, textures, etc., and more complex features built out of these.

(2) *Psychological Features:* those observable or experienceable by each person for himself, *e.g.* his own feelings, emotions, purposes, and so on.

(3) *Occult Features:* those (if any) discoverable in some other way, but not necessarily discovered, or even thought to be discoverable, by everybody and which some people may never discover, *e.g.* the signs of God's activity, the presence of a Person, and so on.

Now it is the discovery of these features which leads us to want different descriptions of the situation. One man examining a situation external to him finds only the physical features and so is satisfied with the scientific description; another discovers also the occult features and so wants a religious description *as well.*

(*b*) *The Mechanical and Mental Descriptions of Human Behaviour*

The same distinctions must be made here. The situation must be defined physically if we are to agree about it. One who notices only the physical features, an observer, is satisfied with a mechanical description of a piece of behaviour; one who notices also the psychological features, an actor, wants a mental description *as well.*

This interpretation of both pairs of descriptions may be conveniently illustrated by one example. Consider an act of charity. The situation is one upon which we can all agree through ordinary observation. We see a man watching a hungry woman begging, hear him told that she has a disabled husband and five children, see him look in his note-case and hand the woman a pound note. Since we all observe these things, and could observe many more of the same kind if we examined the situation more closely, we all want to give a scientific, mechanical description involving only the physical features. But the charitable man himself has been conscious of purposes and decisions, inner conflicts and choices, and would want to include these in a complete description of the situation. He would want, that is, to give a mental description as well, involving the psychological features. But any of us might find here the working of a Divine purpose or the presence of a Person, and whoever did so would want to give, also, a religious description, involving occult features. These descriptions would then be said, at least by those who accepted them, to be all necessary and all to refer to the same situation, as 'situation' has now had to be defined, but not to be equivalent to one another.

It seems that something like this must be meant by MacKay and Coulson when they introduce the notion of complementarity. That is, if they say that the religious and scientific descriptions are complementary descriptions of the world, they must mean by 'the world' the totality of physical features. I shall refer to this definition of 'complementarity' as 'the new definition'.

It should be said, at this point, that a possible objection to my statement of the view is that I have taken a naïvely realistic view and that the theory is more plausible if a different view is taken. Both MacKay and Coulson are inclined to talk of the scientific and religious descriptions as descriptions of our experiences. But however satisfactory this may be for science, and even this is debatable, it surely will not do for religion. It is of the essence of religion that certain experiences are signs of Divine activity; we cannot take religious experiences seriously, continuing in a religious belief, without believing that they indicate something beyond themselves. There is no need to consider what might be called 'religious phenomenalism'.

The new definition is obviously different from my definition of 'complementarity' because when I said that complementary descriptions refer to different features of one situation, I was using 'features' and 'situation' in what I take to be their ordinary senses, according to which the features of a situation are part of that situation and the situation is just the sum of its features. That is, the situation would be composed of, and defined by, its physical, psychological and occult features, etc. But here, the meaning which must be given to 'the situation' is 'all its physical features', since these define the situation from which we start in agreement. Thus the psychological and occult features are only features *of that situation* in a very peculiar sense. But otherwise one who had not discovered the occult features would never be able to agree with one who had on what the situation was.

It might be said that we could agree to mean either of these two things by 'complementary descriptions', according to our purpose, but this seems to me to do violence to language in an unhelpful way. The words which acquire new uses, if the new definition is accepted, are 'feature', 'situation', 'description', 'about' and some related words.

(*a*) '*feature*' *and* '*situation*'

We are asked to consider features of a situation which are not parts of it. This seems no more helpful, in our present difficulty, than talking of different situations with different features. If to say that the scientific and religious descriptions

are of the same situation is to say that the first is of the physical features and the second is of the occult features, or the physical plus the occult features, of a situation physically defined, then the original difficulty appears in another place. We avoid saying that there are situations with which science cannot deal only at the expense of saying that there are *features* of certain, or all, situations with which science cannot deal. We can now correctly say that science can deal with every situation and that the two descriptions deal with the very same situation, but we have to add that they deal with different *features* of these situations and that some of these features are of a kind upon which we cannot get agreement. It is obvious that we lose more than we gain by this curious procedure.

(*b*) '*description*' *and* '*about*'

We usually think that a description of x must be about x and not something else. But note what happens here. "Two descriptions, scientific and religious, are of, or about, the same situation." The scientific description is really about the situation since this is physically defined, whereas the religious description is about something else (as well ?), namely, features which are not part of the situation. The two sentences "Oxygen is made by heating potassium chlorate" and "Oxygen is made by the electrolysis of water" are, in a sense, about the same thing, oxygen, but in a much less trivial sense they are about quite different things, heating potassium chlorate and electrolysing water. It would be odd to say, without qualification, that they were about the same thing. In a similar way, the sentences "This piece of overt behaviour is the effect of causes x, y, z, . . . ", "This piece of overt behaviour is the result of decisions, choices, . . . " and "This piece of overt behaviour is the result of an original act of creation of this and that sort" are about the same thing in a trivial sense, but about quite different things in an important sense. Similarly, to return to an example of MacKay's, the description of ink-marks is about ink-marks, but a description of the algebraical problem they embody is about something else, what the ink-marks mean or symbolize. "This flag is red" is a description of the flag but "This flag means 'stop'" is a description of something else as well, if, indeed, it is properly said to be a description at all. Just as it is odd to say that religious statements are descriptive, it is odd to regard an account of the meaning of signs as a description. However, I have said that my main purpose is to discuss the use of 'complementary' rather than the use of 'description' and I shall therefore not dwell upon this point.

The analogy between the religious description and the description of the algebraical problem is perhaps more damaging to the theory than helpful. The inventory of the ink-marks does leave out the algebraical problem unless it mentions what the ink-marks *mean*, and then it would not be a mere inventory of the ink-marks but a statement also of certain conventions or decisions which are not recorded in them. We should not understand the algebraical problem from a description of the ink-marks alone. In a similar way, the scientific description describes facts accessible to all of us but these facts have to be seen to be signs of something else before it can be seen that they are the material for a religious description. At this point the analogy breaks down and gives even less support to the theory, for the facts are claimed to be signs *really* and not by convention. If a person fails to see the algebraical problem in the ink-marks, we can simply explain to him that this is what they mean and can be perfectly satisfied if he accepts this ' for the sake of argument '. We should think it inappropriate if he asked whether this were true or whether they really meant this, intending to ask more than whether this is what most mathematicians mean by them. On the other hand, one who put forward the religious description would be far from satisfied if an opponent agreed to accept it for the sake of argument and he would think it appropriate if the opponent asked whether it was a true description, meaning to question more than its general acceptance by religious people. The ink-description is of little use without an added account of what the ink-marks mean: the scientific description is of a great deal of use without the religious description.

It seems, then, that very little, if anything, is gained by talking of the religious and scientific descriptions of the world or the mental and mechanical descriptions of human behaviour as complementary, on either of the two definitions. If my definition is used the chief difficulties remain, while on the new definition the conflict is shifted to another place and several words have to be used in unusual and confusing ways. Evidently some other means of dissolving the conflict is needed.

4. *Complementarity in Physics*

Before considering what is needed to dissolve the conflict, I should like to discover where the idea of complementarity is used in any clear sense. The obvious example of its use, in my sense, and probably the only one, is Niels Bohr's *Principle of Complementarity*. It seems likely that the authors of the view I am considering were led to it by the currency and success of this

principle in physics. But what has led them to it is less important than the suspicion that they hope to draw support from the physical theory. A brief examination of this theory may throw some light on the usefulness of the idea of complementarity in other connexions.

Bohr advanced his principle [1] to remove a conflict. Investigations of atomic phenomena had shown that one coherent language could not be found to describe all the behaviour of fundamental particles under different conditions. Two languages, one referring to particles and the other to waves, were needed to give a complete description. The same was true of optical phenomena. Certain *observable* phenomena, such as interference and diffraction, appeared to demand description in terms of waves and their description in terms of particles led to anomalies; other observable phenomena, such as the Compton effect and Wilson tracks, appeared to demand description in terms of particles and their description in terms of waves led to anomalies. To understand all the phenomena both descriptions were needed but they seemed inconsistent and to imply contradictory statements such as "β-particles (or light or . . .) behave sometimes as waves, sometimes as particles". But a wave cannot be, or behave like, a particle and *vice versa*. Bohr suggested that the two descriptions complement one another, which meant that they must be kept separate and each used in the appropriate situations. Because they cannot be mixed they cannot contradict one another, but they are necessary *in principle* since the observations which support one make impossible the observations which support the other. Thus both are needed to describe all the experimental situations, while in any given situation one is appropriate, the other is not.

Bohr does not state very clearly the meaning he attaches to 'complementarity' and commentators disagree about this. However, it seems that his reason for using the word is that when physicists came across the anomalies they thought they were experimenting with the same 'thing' (light or electrons or . . .) under different conditions and were trying to describe the same 'thing' (the behaviour of light or electrons or . . .) under different conditions. Moreover, there is still good reason for thinking this. Ordinary experience suggests that the same

[1] *Nature* (1928), vol. 121, p. 580; *Phys. Rev.* (1935), vol. 48, p. 696; N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge, 1934. Lecture by Bohr, Congress for the Unity of Science, Copenhagen, 1936. See *Philosophy of Science*, vol. 4, p. 289. See also Heisenberg, *The Physical Principles of the Quantum Theory*, 1930, and Reichenbach, *Philosophic Foundations of Quantum Mechanics*, Los Angeles, 1944.

thing enables us to see, produces rainbows and acts on photographic plates; 'scientific' experience shows, for example, that light *from the same source* behaves in different and apparently anomalous ways according to what we do with it. The same sort of thing can be said about the fundamental particles. Indeed, only because this is so are we justified in using the word 'complementary'. If we took no notice of these facts we should be perfectly correct in saying that the wave and particle descriptions were of different situations between which we have discovered no connexion, and there would be no need for the idea of complementarity. On the face of it this does seem to be an example of complementary descriptions, in my sense, if we can agree that the wave and particle descriptions are in some relevant sense about the same situation.

On my definition the situation is, for instance, the total behaviour of β-particles. Some physical features, their observed behaviour under certain conditions, require the wave description; other physical features, their observed behaviour under certain other conditions, require the particle description. All these features are parts of the total behaviour of β-particles and so of the situation. Thus, if it does not stretch language too much to call the total behaviour of β-particles 'the situation', this use fits my definition of 'complementary descriptions'.

It is difficult to see how it fits the new definition, if we bear in mind the reasons for which that definition is put forward. We cannot say that the two descriptions are of the same situation on this definition and even if we begin by saying this we shall be forced, in the end, to admit that they are about different situations. This is a consequence of accepting a definition of the situation in physical terms in order to allow agreement about what the situation is. On the new definition what makes one situation differ from another is different physical features and the reason for regarding the descriptions as complementary, rather than as independent, was that both descriptions were about *all* the physical features of the situation. Here we are faced with the fact that the particle description just is not about the physical features we call 'interference' and the wave-description just is not about the physical features we call 'the photoelectric effect'. One description is about what the other leaves out, whereas the claim about the scientific and religious descriptions was precisely that one is *not* about what the other leaves out. This is perhaps just another way of pointing out that the conception of features of a situation which are not part of that situation, as its physical features are parts of it, is unsatisfactory.

This difficulty does not arise on my definition. There is no reason why one description should be about all the features, whether physical or not, of the situation. The wave and particle descriptions are complementary just because one of them is about some of the physical features and the other about other physical features of one situation and, of course, a statement is about the situation even if it is about only one of its features. It seems, then, that the new use of ' complementary ' is really a new use and cannot derive support from the physical theory.

5. *Further Objections and the Removal of the Conflict*

The theory of the complementarity of science and religion seems to be put forward to meet two criticisms which I shall put rather crudely for the sake of brevity.

(*a*) Against the religious account: Science can give a complete description of the world, as far as that is possible, so that nothing remains for religion to describe.

(*b*) Against the scientific account: Science has, by claiming to deal with everything, argued God out of the universe, so that science must have gone wrong somewhere.

The theory attempts to answer these by admitting that science can, in principle, deal with everything, claiming that the religious description deals with everything but from a different point of view and adding that to understand anything completely we must see it from both points of view. I have shown that on neither definition of ' complementarity ' does the theory really support this. It only *appears* to allow science and religion to deal with the same things. Now I want to argue that calling the descriptions ' complementary ' is misleading in other ways and, moreover, quite unnecessary.

The analogy with Bohr's theory suggested by the use of the word ' complementary ' is misleading in an obvious way. The evidence for the wave and particle descriptions is logically of the same kind and it is evidence that we all could have, all could be shown. The conflict arises because we can all agree that both descriptions are necessary and ' true ' according to the same scientific test of truth. That is, we *start* by seeing that both descriptions are needed and the idea of complementarity is introduced after they are established to show how they can both be accepted, to show that they do not conflict in spite of first appearances. There is no argument about the evidence.

With the religious and scientific descriptions it is the other way about. We are not all agreed upon the need for both descriptions. One who objects to the religious description does not

do so because he fails to see how certain evidence for it can be squared with the evidence for the scientific description, but because he fails to see the evidence for it, or, what amounts to the same thing for him, because what he does see does not look like evidence to him. The idea of complementarity can do nothing, or very little, to bring him to see; the most it could do would be to show him that the two descriptions could be given without inconsistency if both were necessary. Moreover, the idea of complementarity is unnecessary if the religious description is unnecessary. The theory could thus, if it worked, be a reply to only the most naïve of criticisms and would leave the more difficult one still to answer, for it leaves open the question of the necessity for the religious description.

Indeed, I am surprised that anyone who accepts the religious description should be worried by this sort of criticism, for the shorter and less misleading and obviously correct answer is that science cannot claim to deal with everything. If the religious description is correct, it is surely by definition about what science cannot deal with, for it deals in part at least, with what is by definition non-physical while science, by definition, deals with what is physical, in the wide sense of being directly or indirectly verifiable in sense experience. The two descriptions are, as MacKay seems to have seen, about things in different logical categories, there cannot be, and never could be, a conflict, and so no purpose in introducing the idea of complementarity. More than this, they could not sensibly be called 'complementary' if this is admitted since this is to admit that they are about different situations. It follows that to fear that science will one day deal with what is now the field of religion is irrational. It is to fear that what is by definition non-physical will become physical, or that science will become something else. It is irrational, then, to fear that science will argue God out of the universe because the proud claim that science deals with what is publicly observable has the consequence that its arguments and conclusions have no bearing either way on the existence of a God who is by definition inaccessible by such means. The answer to this sort of criticism in terms of complementarity gives away too much to the critics and all to no purpose.

Of course, there are serious conflicts between science and religion *if* we take literally, for instance, statements in the stories of miracles. "Five loaves and two small fishes fed five thousand" certainly conflicts, taken literally, with "Five loaves and two small fishes couldn't have fed five thousand—there wouldn't be enough to go round, they wouldn't provide enough calories,

vitamins, etc., to nourish, or even satisfy, five thousand people ". Both look like statements with which science can deal and that is why they conflict if they are taken at their face value. But it is not an indispensable prerequisite of religion that such statements should be taken at their face value, while the objective existence of a God or Gods is. Thus the theory is aimed at alleged conflicts which would be, if they were conflicts, far more serious and fundamental than these. For example, that between " This behaviour is the result of causes x, y, z . . . " and " This behaviour is the result of God's original act of creation " or " This behaviour is a consequence of the presence of a Divine spark in men ". But the second and third statements do not even begin to look like statements with which science can deal, when science and religion are properly understood. Science has no means of talking of such acts or sparks, either to affirm or deny them. That is why such statements do not and could not conflict. If we misunderstand these statements, or misunderstand the nature of science, we may think they conflict, but merely saying that they are complementary hinders understanding by obscuring the differences instead of illuminating them. To see that they do not conflict is to see that there is no need to regard them as complementary.

At this point, Occam's Razor might be invoked in two ways.

(*a*) A given situation is adequately accounted for by science, so there is no need to multiply entities by giving another account of it, mentioning God. But the reply to this is that the religious account attempts to answer questions not even posed in science, *e.g.* " Why is all this as it is ? "

(*b*) Religious experiences can be adequately explained psychologically, so there is no reason to suppose that they have other than a psychological origin. But the reply to this is (i) that those who have had such experiences stubbornly refuse to abandon the view that they have special knowledge which others have not, and (ii) the fact that such an explanation can be given does not imply that no other explanation is valid, does not imply that the putative signs are not signs. The religious hypothesis allows that God arranged everything in such a way that a mechanism could always be discovered for it in the scientific way.

The unreality of the conflict should be obvious from the behaviour of religious persons when they get into difficulties over this, and from the kind of difficulties they get into. When observed facts refuse to fit into a scientific description, we eventually reject or modify the description. But certain parts

of the religious description are fixed in advance of the facts of ordinary experience, for instance, God's characteristics. If the facts appear not to be consistent with these, there is no question of rejecting the view that God has these characteristics or, *a fortiori*, exists. The problem of pain and evil is a problem about how pain and evil, admittedly present, are consistent with there being a benevolent God, and *not* whether, in view of pain and evil, we can after all assert God's existence and benevolence. The difficulties are different. Something would count against the scientific description but nothing, at least nothing of that sort, would count against the religious description. This is because the scientific description deals with facts and the relations between them and is determined by the facts, while the religious description deals with the relation of the facts, *whatever* they are, to something that is not a fact in the same sense. The religious description deals with the existence and contingency of the facts rather than with their character. Even if the facts *suggest* the religious description, they could have been quite otherwise and still have suggested it. " . . . He maketh his sun to rise on the evil and the good, and sendeth rain on the just and the unjust " (Matthew, v. 45). We can ' meet a Person ' in *any* kind of situation and that ' verifies ' the religious description but only certain kinds of situation verify the scientific one.

The distinction may be made in another way. The scientific description describes ' the facts ' about which we can all agree and these alone are what demand the description, but what demands the religious description is more than just these facts and what it describes includes more again than this. We see signs of God and not God but the description talks about God: we find contingency but we talk about necessity. What the scientist would call an interpretation the religious person calls a fact or datum. In the ordinary sense of ' see ' we see the same things, but one who gives the religious description sees them as signs of something beyond themselves, while one who rejects it regards the claim that they are signs as an interpretation which it is unnecessary to give. Moreover, whatever scientific, psychological account he gives to explain the fact that some people see them as signs has no bearing whatever on whether they are signs or not, just as the discovery of the correct answer to a mathematical problem by an invalid method has no bearing on the correctness or incorrectness of the answer. But the religious person has no doubt; he ' sees ' signs and his description is not just of what we all see, nor just of what he

' sees ', but also of what the signs are signs of and of the relations between this and what we all see. To borrow an illustration from Professor Wisdom, the scientific account of how the garden grows is supported by facts upon which we can all agree. One who gives the religious account says "Ah, but that is not all. Don't you see the working of a purpose in the pattern ? Don't you see signs of the invisible gardener, the *really* invisible gardener ? " and if we ask " Where ? " he replies, " Everywhere ". And to the scientist, as scientist, this looks like an interpretation and he can truthfully reply " No, and yet I see all there is to see ". If the religious description is about these things we all see, it is also about something else, indeed, primarily about something else without which it would be unnecessary. If we call this something else a ' fact ' we must recognize that we use the word in different senses.

The idea of complementarity cannot do more, in this connexion, than show how both descriptions may be given. Those who put it forward appear to be more optimistic about its powers than is justified. MacKay says, " If both are supported by the facts and consistent with one another . . . then both are true and that is that ! " The question of the truth of the religious description still raises the familiar difficulties, as indeed it must on any solution of the conflict, since the religious description is not supported by the facts in the sense, understood by all of us, in which the scientific description is. The truth of the religious description is an open question until we understand the meaning of ' supported by the facts ' and ' the facts '. As I have argued, the facts which support it are not of the same kind as those which support the scientific one and, judged by the criteria of science, they are and must be queer, that is, queerly called ' facts'. It should be obvious, too, that although the wave and particle descriptions are true in the same sense, this cannot be said of the scientific and religious descriptions. On these points alone, MacKay's conditions for truth are odd and the comparison misleading.

What can be said about consistency ? In what sense can the religious and scientific descriptions be consistent with one another ? We can, if we like, say that if p is not inconsistent with q then they are consistent, even if they *could not be* inconsistent. But if they could not be inconsistent do we say anything ? Two statements in different logical categories could not be inconsistent and so are only vacuously said to be consistent. This could be no help in establishing the truth of either. The mere fact that it is possible to put forward a theory which does not conflict with anything, facts or descriptions, because logically

it could not, and which relies on facts which we do not all seem to see, or experiences which we do not all seem to have, does little to establish the truth of the theory for the unfortunate ones. This is not to say that the religious account is not true, but only that the idea of complementarity, at its best, leaves intact the greatest barriers to seeing its truth.

The three pairs of descriptions, namely, the religious-scientific, mental-mechanical, wave-particle descriptions, are even less alike than I have so far suggested. The mental and mechanical descriptions resemble the wave and particle descriptions more closely than do the religious and scientific descriptions and this may make it more helpful to call them 'complementary' but not, I think, much more helpful. The evidence for the wave and particle descriptions is of the same logical kind and is observable in the ordinary way by all of us. The evidence for the mental description of behaviour is in some sense private to each of us. Although we cannot observe each other's purposes, we can all understand an account, in terms of purposes, of another's behaviour because we have all been aware of our own purposes. This differentiates this pair from the religious and scientific descriptions. We all have the material for giving the mental description but we do not all have the material for giving the religious description, or, if you prefer, some people think they do not. They feel, not that they simply need more evidence of a kind familiar to them, but that they have *no* evidence of the requisite kind, that they do not even know what 'evidence' means here.

The way out of the alleged conflict, then, seems to be through the recognition that scientific and religious statements are in different logical categories and so could not possibly conflict. Religion claims to deal with something forever outside the field of science, defined as inaccessible to science, namely, the relation between God and the natural world. The 'facts' upon which it rests and its canons of evidence are different from those accepted in science and this alone establishes the independence of the two disciplines. It does not follow that the religious description has no bearing upon the natural world, since God's immanence in the natural world may be part of this description, but it does follow that neither is in danger from the other and that the idea of complementarity is unnecessary and misleading. MacKay says, "Perhaps most of all I want to dispel any impression that one's faith and one's science have to be kept safely insulated from one another, each in its own private preserve. The really interesting thing to me is the way in which they each sparkle into more meaningful life as one deliberately seeks to interrelate them."

Even this is surely safeguarded if what science studies can be seen as a consequence of what religion studies.

I should like, finally, to make a more general point about the use of the idea of complementarity. To apply it to the religious and scientific descriptions and the mental and mechanical descriptions is to suggest that the members of each pair are more like one another than they can possibly be and as like one another as are the wave and particle descriptions. This may lead us to ignore important differences and to look for the wrong kind of evidence. Moreover, it looks rather like a device for concealing a gap in our knowledge which it might be possible to fill. This is so even of Bohr's use of it. There is a danger of saying " We have reached an *impasse* and instead of trying to go further when we have shown this to be theoretically impossible, let us give the descriptions a label that will remind us not to try and so prevent us from breaking our heads ". This conceals the fact, without helping us to understand it, that we want to say *both* that the two descriptions are about the same thing *and* that they are about different things and may have the effect of discouraging us from enquiring into the meaning of ' theoretically impossible ' in this context. It means, of course, ' impossible on the accepted theory '. Perhaps the accepted theory is the only, or the best, possible one, but we only discover that by trying to find others. It is this attempt that is in danger of being stifled by the Principle of Complementarity. This has not happened in physics and there are promises of a unified theory ; [1] in other contexts, where the incentives to continued investigation are weaker, there is much more likelihood of its happening. We are usually profoundly worried by the suggestion that two descriptions of one situation are both needed and our reaction is to attempt to show that one is superfluous or that they are alternatives. MacKay thinks we should allow the idea of complementarity to dispel this worry : Dr. Oppenheimer seems happy to find complementarity wherever he looks.[2] Perhaps, as MacKay says, the ' perception of complementarity ' is something to be striven for, but we need more precise criteria for complementarity than we have been given, since, lacking such criteria, there is a danger of our putting it in rather than perceiving it, so that ' complementary descriptions ' may become a label on a rag-bag to which we consign our unsolved problems. It is easy to prefer comfort to industry : it is painful but perhaps essential that we should break our heads.

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[1] See, *e.g.* A. Landé ; *Quantum Mechanics*, 1951.
[2] Reith Lectures, 1953. *The Listener*, L, 1290-5, 1953.

II.—ARE MORAL PROBLEMS GENUINE?

By D. H. Monro

By moral problems I mean, primarily, the basic questions of moral philosophy—such questions as: What is the ultimate justification of morals? Is morality objective or subjective? Are sentences containing ethical terms to be regarded as in the indicative mood, in the imperative, or in the optative? I think it is particularly easy in our time for the philosopher who asks himself such questions to feel that he is beating his head against a brick wall. For, in the first place, he is faced by the logical objection to any form of naturalism: the impossibility of deriving "ought" from "is"; secondly, he is unlikely, in the prevailing climate of opinion, to feel satisfied with traditional forms of non-naturalism: with a transcendental world of values, for example, or with simple, unanalysable, "non-natural" qualities; thirdly, he is likely to feel that subjectivism, in any of its forms, fails to do justice to the way people actually think and behave. All the traditional answers, then, are unacceptable; and yet there seems no other possible answer.

Now in this situation it is tempting to suppose that the answers are unacceptable because there is something wrong with the questions. Are they perhaps questions that do not admit of an answer, like the child's "why"?—(Why do kittens grow into cats, and puppies into dogs, Daddy?). And, of course, some modern philosophers have developed a technique for deciding when questions are genuine and when they are pseudo-questions. First, they say, those who ask pseudo-questions are not genuinely puzzled. Or, at least, it is hard to see what they are puzzled *about*. What is it about kittens and puppies, for example, that the child finds puzzling? Secondly, nothing that could possibly be offered as an answer to the question is likely to count *as* an answer. The most detailed technical description of the stages of growth in puppies and kittens, for example, will still leave the question unanswered. And what else is there that could possibly be said? Thirdly (and this does not apply to the question about kittens and puppies), where alternative answers are offered, and are disputed, the dispute turns out to be, like the perplexity, unreal. The disputants, that is to say, are really disagreeing about the proper form of words to apply to phenomena about the nature of which they are in perfect agreement.

Realists and nominalists, for example, already know (it would be claimed) all that there is to know about particulars and universals. When the one party says that the particular participates in the universal, or the other party says that the universal is abstracted from the particular, they are differing, not about the facts, but about the proper form of words to be used in describing the facts.

Now when we apply these tests to the problems of moral philosophy, it is quite easy to make out at least a *prima facie* case for their being pseudo-problems. For what, after all, is the moral philosopher puzzled about ? When he mutters distractedly : " I don't know whether sentences containing ethical terms are to be interpreted as assertions of fact, expressions of feeling or simply commands " it is possible to answer : But you do know. When the local parson approached you yesterday about signing a petition for the removal of the state-controlled totalisator agency in your suburb, you knew exactly what he was saying. You did not really imagine that he was ordering you about, or calling your attention to some matter of fact, or pouring out his wishes. You know what sort of consideration had led him to say : " Gambling ought not to be encouraged " and what kind of argument he would consider relevant. You were not in the least in the position you found yourself in the night before, when you went to a lecture on atomic physics and found that you did not know what the lecturer was saying or how his statements were to be interpreted. And, we may go on, since neither you not your fellow-philosophers are really perplexed, you do not really disagree either. You both know in what respects " X is good " is like " X is yellow ", in what respects it is like " I like X ", and in what respects it is like " Would that X existed more often ! " You also know in what respects it differs from each of these. The question you persist in quarrelling about is : But which is it most like ? And there is really no room for this question once we have a complete enumeration of all the points of resemblance and all the points of difference.

Further, if the question is : But how can moral judgments be justified ? it may be asked : What would you accept as justification ? We can tell you what kind of arguments are actually used and accepted. We can even show you what principles you yourself actually apply when you ask yourself : ought I to do this ? If you persist in asking : yes, but am I really justified ? what more is there that can possibly be said ?

At least at first sight, then, the problems of moral philosophy

would seem to conform to the criteria for pseudo-problems. Furthermore, it is possible to draw a close parallel between these problems and the problems of induction; and the philosophers who talk in this way have already declared the problem of induction to be a pseudo-problem. To say, with Hume, "'Tis not contrary to reason to prefer the destruction of the whole world to the scratching of my finger" is like saying (with Hume) that it is not contrary to reason to suppose that the sun will not rise tomorrow. Both statements have a wildly paradoxical air; and we can say of both that the philosopher does not mean what he says, or at least what any ordinary man would suppose him to be saying. What he does mean is that the reasoning on which we base our conclusions in these matters is quite different from the reasoning we employ in (say) mathematics. This is worth pointing out; but when it has been pointed out there is really nothing more to say, except of course to describe in detail the methodology of science or of morals. It is quite pointless to complain that we can find no better reasons than those we do in fact appeal to; for nobody (including the philosopher himself, when he is not being perverse) really wants any better ones. Those we have are quite good enough.

This method of treating the problems of moral philosophy is adopted, quite explicitly, by Mr. Toulmin in his book *The Place of Reason in Ethics*, and, although I am not sure whether one should attribute the same view to Mr. Hare, there are at least one or two passages in *his* book, *The Language of Morals*, which do lend themselves to this interpretation. In particular, there is the following passage:

> Thus a complete justification of a decision would consist of a complete account of its effects, together with a complete account of the principles which it observed, and the effects of observing those principles—for, of course, it is the effects (what obeying them in fact consists in) which give content to the principles too. Thus, if pressed to justify a decision completely, we have to give a complete specification of the way of life of which it is a part. This complete specification it is impossible in practice to give; the nearest attempts are those given by the great religions, especially those which can point to historical persons who carried out the way of life in practice. Suppose, however, that we can give it. If the inquirer still goes on asking "But why *should* I live like that?" then there is no further answer to give him, because we have already, *ex hypothesi*, said everything that could be included in this further answer. We can only ask him to make up his own mind which way he ought to live; for in the end everything rests upon such a decision of principle. He has to decide whether to

> accept that way of life or not ; if he accepts it, then we can proceed to justify the decisions that are based upon it ; if he does not accept it, then let him accept some other, and try to live by it. The sting is in the last clause. To describe such ultimate decisions as arbitrary, because *ex hypothesi* everything which could be used to justify them has already been included in the decision, would be like saying that a complete description of the universe was utterly unfounded, because no further fact could be called upon in corroboration of it (p. 69).

So far, then, it would seem that the proper procedure is to examine the reasons which we actually give when asked to justify a moral decision. (There is, it is true, an awkward question here : Who are *we* ? But let us put this aside for the moment.) What does count as " a good reason " for ethics ? Well, to begin with, Toulmin tells us, in an Aristotelian Society paper,[1] that " I ought to do this because I promised to " is a good reason. We appeal to the principle : " promises ought to be kept ". And Toulmin is even prepared to say (subject, as we shall see, to a qualification) that to go on asking " why " is to behave like the child who asked " Why do kittens grow into cats ? "

Now when a child does ask such questions, we are generally reduced to saying something like : " Well, that's just how things are." We point, that is, to some statement of fact which can only be accepted or rejected. The question " why ? " is out of place, though the question " how do you know ? " may not be. And, of course, moral philosophers who have argued in what seems to be Toulmin's way about promise-keeping have supposed themselves to be pointing to a fact (though a rather unusual kind of fact) and have devoted a good deal of attention to the question : " how do we know ? " They have usually advanced a theory about intuition. But this, Toulmin tells us, is a mistake. It is a mistake because philosophers have been misled about the " logical grammar " of the word " know ". " How do you know ? " is not a psychological question, but a logical one. Knowing is not a mental act (" cognising ") and to ask " how do you know ? " is not to ask for a special mode of cognition (" intuiting "). " How do you know ? " means " What are your grounds for ? " And when we ask this question, Toulmin tells us, we should always remember that it is perfectly proper, on occasion, to say : " There are none, because none are needed." That is to say, there are some assertions which are not themselves grounded, because they serve as the grounds of other assertions.

[1] Aristotelian Society, *Proceedings*, v, 50 (1950), pp. 139-156.

In arithmetic, for example, $1 + 1 = 2$ is such an assertion. If anyone challenges it, the answer is that not to accept it is not to do arithmetic. And to substantiate this we need only challenge the dissentient to produce an alternative arithmetic based on some different axiom. Similarly, if anyone challenges basic moral principles, the answer is that not to accept such principles is not to "do ethics". To quote Toulmin: "If the question 'How do you know promises ought to be kept?' is to remain in the *logical* field, all we need do is parry it with the question: 'And what have you in mind as an alternative?'"[1]

Now the answer to this question might seem obvious enough. "What I had in mind was to break my promise. I often do." But I think that Toulmin would say that this answer would be irrelevant. The question is, not whether you do in fact break this moral rule on a given occasion, but whether you can seriously put forward an alternative system of ethics (or moral code) based on or including the principle: "promises need not be kept." Now this suggests that a moral principle must conform to certain formal criteria. As a minimum, it must somehow be capable of cohering with other moral principles so as to add up to something called a way of life.

Let us see in rather more detail what these criteria are taken to be. (They are, I would suggest, implicit in what Toulmin and Hare have to say, though not as a rule explicitly stated by them.)

(1) A moral decision is a decision of principle. This means that it must have some degree of generality. I have stated this vaguely, because a principle may be "general" in many senses, and it is important to distinguish between them.

(*a*) Any action may be regarded as having what Kant called a "maxim". And we are tempted to put this by saying that to decide to do a particular action is, in a sense, to decide to act in the same way in all similar circumstances. But this is misleading; all that we can really say is that there is a motive for the action. If I decide to break a promise, and my motive is (*e.g.*) to save myself expense, my action can be said to conform to the principle: Promises are to be broken when keeping them involves expense.

But to say that my action conforms to this principle is not to say that I will follow this principle consistently, *i.e.* that other actions of mine will never be found to conform to the contradictory of this principle. Nor does it mean that I consciously formulate this principle to myself. Still less does it mean that

[1] Aristotelian Society, *Proceedings*, v, 50 (1950), pp. 139-156.

I advocate the principle, *i.e.* advise others to follow it as well as following it myself. We have here two other senses in which a principle may be said to be general:

(*b*) We must apply it in all similar circumstances.

(*c*) We must approve of others applying it as well as ourselves.

Hare would say, I think, that a decision of principle must be general in all three of these senses. He gives us an elaborate analogy with learning to drive. To learn to drive is to learn principles of driving, or (and this is not really different) to acquire driving habits. That is to say, we do not learn what to do in this situation merely, but in all situations of this kind. Words like "good", "ought", etc., have the special function of imparting instruction (which may be self-instruction) and this is why we cannot say X *is good* unless we are prepared to say that Y, which is like X, is also good. The only difference between moral principles and (*e.g.*) the principles of driving is that they apply to a wider field of action. As Hare puts it, you can get out of driving cars; you cannot get out of being a man. Learning moral principles, then, is learning how to live; *i.e.* making the decisions of principle which add up to a way of life. And Hare is even prepared to say that this is the difference between being a good poisoner and being a good man; between the sentences "you ought to give a second dose" (said to a poisoner) and "you ought to tell the truth".

> We cannot get out of being men; and therefore moral principles, which are principles for the conduct of men as men—and not as poisoners or architects or batsmen—cannot be accepted without having a potential bearing upon the way that we conduct ourselves. If I say to a certain person "You ought to tell the truth", I signify my acceptance of a principle to tell the truth in the sort of circumstances in which he is; and I may find myself placed unavoidably in similar circumstances. But I can always choose whether or not to take up poisoning or cricketing as a profession. This is bound to make the spirit in which we consider moral questions very different from that in which we consider how we ought to poison Jones, or build him a house; but the logic of the word "ought" is not markedly different in the two cases (p. 162).

It seems to be assumed that we cannot live at all without adopting a "way of life"; *i.e.* without consistently following principles that are general in all three senses. And this is open to question. We can hardly avoid acting from general principles in sense (*a*): *i.e.* our actions have motives. But it is hardly true that we must always act from general principles in sense (*b*): *i.e.* principles we are prepared to apply in all similar circumstances. There is doubt about this even if we refrain from asking awkward

questions about the word "similar". We all know the kind of person who is filled with enthusiasm one day for, let us say, the ascetic life, and is prepared to act from such principle as: "One ought to abstain from all luxuries" and who is equally enthusiastic next day (or next week or next year) for a diametrically opposite ideal of gracious living. It may be objected that this is to change rapidly from one principle to another, which is different from not acting from any principle: that at the moment of decision this person is prepared to act in the same way in similar circumstances, even though he does not in fact act in this way when the circumstances next arise. But I do not know that this is really different from simply acting from one motive on one occasion and from another in another occasion, which would be to act on principle in sense (*a*), but, not in sense (*b*). But it may be admitted that even the most unstable of us, in so far as we are creatures of habit, and in so far as we are influenced by what we have learned, do follow consistent principles of action most of the time. What really raises my doubts is the assertion that we always act from principle in sense (*c*): *viz.* principles that we apply to others as well as to ourselves. And here it becomes necessary to ask: what are similar circumstances? Hare avoids a difficulty when he says that it is a rule of the use of the word "good", that, if X is good, and Y is exactly like X, Y must be good. For in practice X and Y are never exactly alike; and the difficulty is to decide what differences are relevant. Does it matter that X is an action performed by A, and Y an action performed by B? I think Hare would reply that principles would be useless in teaching unless they could be applied by anyone. The principle must at least take the form: "Those in A's circumstances should do such-and-such"; even if A's circumstances are in fact unique. But we still need to ask: Does it matter if A and B belong to different social classes, or different races, or have different coloured hair? If I say to my friend: "You ought to tell the truth, because you are red-haired, and red-haired people ought to tell the truth", I need not be deterred by the fear of finding myself in the same circumstances, if my hair is not red.

(2) I think, then, that Hare is really appealing to another criterion, one which is quite specifically stated by Toulmin. "If", says Toulmin, "the most general principles to which we can appeal still contain some reference to us, either as individuals or as members of a limited group of people, then our appeal is not to morality but to privilege."[1] Toulmin bases this simply

[1] *The Place of Reason in Ethics*, p. 168.

on the meaning of the word "moral". Hare, in his review of Toulmin,[1] objects to this argument. He prefers to appeal to the function of ethical terms: they are used to impart instruction. But it is not immediately obvious that we cannot teach others to abide by one set of principles while following quite a different set ourselves. Rules of privilege too can be taught. And notice that rules of privilege may be adhered to by both the privileged and the unprivileged (*e.g.* both the red-haired and those not red-haired may accept the principle that only the red-haired should tell the truth). Rules of privilege, *i.e.* are general in our third sense. If Hare wishes to rule them out he must be appealing to a principle like Toulmin's.

(3) I think that Hare and Toulmin would both appeal to a further criterion as well, *viz.* that a moral code or a way of life must be internally coherent; one moral principle must not be inconsistent with another. Toulmin at least seems to be thinking of something like a calculus: he compares moral principles with the axioms of mathematical systems. Here again it is obvious that this is not derived from an analysis of the actual behaviour of human beings. This is not at all what is reported by those social psychologists who have investigated the values of a given community: by the Lynds, for example, in their account of Middletown.[2] It may be protested that neither Hare nor Toulmin is reporting on the way that human beings actually behave: they would not deny that men are unstable, inconsistent and prone to depart from their own principles. They appeal rather to the way men feel they ought to behave. To which it may be replied: first, that Hare suggests that men are forced to adopt principles conforming to the criteria I have mentioned by the mere fact that they are creatures of habit and that habits have to be learned; secondly, that the Lynds too are concerned with what people regard as "good reasons"; and, thirdly, that it is not hard to find people who would dissent from (and not merely depart from) our criteria. There are people who would deplore the attempt to construct a way of life consisting of mutually consistent "principles": they would advocate a life of impulse, and would say that this is a sphere in which logic is quite out of place. However much we may disagree with them, such people do exist. And there are certainly people who would defend rules of privilege.

My point so far is that the concept of a way of life consisting

[1] *Philosophical Quarterly, July* 1951, p. 371.

[2] Lynd, R. S. and Lynd, H. M., *Middletown in transition*, 1937, pp. 403-418.

of actions regulated by principles which are universally applicable, not rules of privilege, mutually consistent, and consistently followed is an artificial one: it is not derived either from an analysis of the actual behaviour of human beings or from their account of how they think they ought to behave. That is why moral philosophers have felt that it does stand in need of some justification. And it is here that the analogy with science begins to break down. It may be said that the methods of science need no justification other than that scientists actually follow them. Not to follow them is not to be scientific, not to "do science", and while individuals or communities are at perfect liberty to be unscientific if they choose, there are some obvious disadvantages from so choosing. They will find themselves: (*a*) unable to understand the world as well as they otherwise might; and (*b*) unable to master certain technical skills. It is by no means clear that any similar disadvantages follow from a refusal to "do ethics". At least it is clear that the first disadvantage does not follow: as Hare very properly insists, a moral code is not an attempt to understand anything; it is a way of behaving. About the second kind of disadvantage there is conflict of opinion. On the one hand, it can be argued that a moral code cannot be justified as a means to the attainment of any end, since a large part of its function is to decide what ends are to be aimed at. On the other hand, it can be claimed with some justification that a moral code constitutes a technique for attaining certain universal ends, such as social harmony, which seems to be necessary if any human aim is to be achieved.

Now so far I have been considering the view that an action is justified if it conforms to a principle, which in its turn conforms to certain formal criteria. We have now discovered a different view, which might also be involved in the analogy with science, *viz.* that an action is to be justified by certain empirical tests.

A few paragraphs back I said that if I say to my friend: "you ought to tell the truth because you are red-haired" I need not fear to find myself in the same circumstances. You may have wanted to object that I might well find myself in circumstances in which I would want a dark-haired person to keep a promise. Hare may have something like this in mind when he says that you cannot get out of being a man. Perhaps he means, not merely that moral rules must be general in form, so that they can be used as guides to action, but also that they must be capable of binding society together, of enabling men to co-operate. Certainly he suggests *some* empirical test when he says: "If he does not accept it (a given way of life) then let

him accept some other, and try to live by it. The sting is in the last clause." I suspect that the test here is not the simple one of practicability : not simply whether it is *possible* to adopt a given moral code, but whether our lives will be satisfying if we do. And this is confirmed by what Hare says about the parents' question : " How shall I bring up my children ? " But it is Toulmin who follows this line of argument quite explicitly, and it is Toulmin, therefore, whom I shall discuss at this point.

Toulmin, although he says that we ought to tell the truth or keep promises simply because " promises ought to be kept ", " the truth ought to be told " count (in ethics) as good reasons, also tells us that these justifying principles can in their turn be justified. We justify them by asking whether they will lead to the harmonising of desires. And we do this because no community can survive as a community unless the desires of its members are brought into some degree of harmony.

> Suppose, for example, that we visit an island, and find that its inhabitants all habitually avoid types of behaviour particularly liable to inconvenience their fellows : then we shall be prepared to refer to the inhabitants of the island as forming a single " community ". And we shall also say that the members of the community " recognise a duty to one another ", and " have a moral code ". But if, instead, we find that we have to divide the inhabitants into two classes, C_1 and C_2—such that members of C_1 are scrupulous only in so far as their conduct affects other members of C_1, but ignore the interests of those of C_2 ; and those of C_2 respect the interests of other members of C_2, but ignore those of C_1—we shall not be able to call them " members of a single community " at all. In fact, we shall call the two sets of people, C_1 and C_2, " separate communities " (pp. 134-135).

And this is, I think, to be connected with another dictum of Toulmin's, already quoted, that " if the most general principles to which we can appeal still contain some reference to us, either as individuals or as members of a limited group of people, then our appeal is not to morality but to privilege ".

It is if course obvious that this is no argument for appealing to morality rather than to privilege. It could provide one only if there were some reason to want C_1 and C_2 to coalesce into one community. I think Toulmin is suggesting that there always will be such a reason, because the members of each community will at some point want the co-operation of the members of the other community. And Toulmin also seems to think that harmonising desires will amount, in practice, to something like the greatest happiness principle of the utilitarians. But we may

grant the point that moral principles arise as the rules according to which men co-operate with their fellows in a community without supposing that these rules will inevitably be such as to satisfy the utilitarian demand for the greatest happiness of the greatest number. Let us suppose that our two communities C_1 and C_2 coalesce in such a way that their members become slaves and slave-owners respectively. No doubt we can no longer say that the members of each group "ignore the interests" of the other group. No doubt there will have to be a code, laying down the duties of slaves to masters and masters to slaves. The code may well be accepted, quite genuinely, by masters and slaves, or at least by the vast majority of them. The result will certainly be to "harmonise desires", in one sense; but not in the sense of putting an equal value on the desires of slaves and slave-owners. The phrase "harmonising desires" is, in short, ambiguous. Because of this ambiguity, I am not sure whether Toulmin would say that we have here one community or two; or whether he would say that the code which knits them together is a set of moral principles or simply a set of rules of privilege. If he says that these are rules of privilege, then moral principles are not the only ones which enable men to co-operate; if he says that this is a moral code, then the criteria to which moral principles must conform are far more varied than he seems to suppose. Either, that is to say, we (or at least the members of *some* communities) can get out of "doing ethics" altogether; or there are several alternative ethical systems, each equally valid.

Now this last view is in many ways attractive. It is particularly attractive to anthropologists. Moral principles, an anthropologist is often tempted to say, simply show us the method by which a given community (or "culture") has chosen to solve the problem of harmonising desires within that community. A member of the community, when trying to decide between two alternative courses of action, will feel that he ought to choose the one which conforms to one of these principles. If he is in doubt about the principle, he may enquire whether it does in fact serve the purpose of harmonising desires within his community. But he cannot go further than this: he cannot ask whether a different community, in which a completely different moral code is in force, is better or worse than his own. Such a question has no meaning. It is possible that Hare means something of this sort when he says that a way of life cannot be justified but only described.

Toulmin does say quite specifically that we cannot judge

between two different ways of life, in the sense of two different "patterns of culture". To ask whether it is better to have one wife, like a Christian, or anything up to four, like a Muslim, is, he tells us, a meaningless question. For marriage is so much a central institution of each society that any change would have many ramifications, so that the question becomes: "Is the Christian or the Muslim way of life the better ?"

> And *this* comparison is, if anything, a private one: which is to say, not that it *cannot* be reasoned about, but that, reason as you may, the final decision is personal. There is no magic wand which will turn the English social system into a Muslim one overnight: the only practical use for the question, "Which way of life is the better ?", is in the service of a personal decision—for example, whether to remain here in our society, such as it is, or to go and live as an Arab tribesman in the desert.
>
> In general, then, if one is to *reason* about social practices, the only occasions on which one can discuss the question which of two practices is the better are those on which they are genuine alternatives: when it would be practicable to change from one to the other *within one society*. Given this, the question, "Which is the better ?", has the force of, "If we changed from one to the other, would the change have happy or unhappy consequences on the whole ?". But, if this condition is not satisfied, there is, morally speaking, *no* reasoning about the question, and pretended arguments about the merits of rival systems—personal preferences apart—are of value only as rhetoric (p. 153).

On the other hand, Toulmin also remarks (in reply to Bertrand Russell, who objected that Toulmin's reasoning about ethics would not convince Hitler): "But whoever supposed that it should ? We do not prescribe logic as a treatment for lunacy or expect philosophers to produce panaceas for psychopaths" (p. 165). Toulmin is not prepared, apparently, to consider Hitler's Germany as providing a "way of life". And this suggests that he is not using the term neutrally, as the anthropologist might, to mean any code actually adopted in any community. To revert to our hypothetical society of slaves and slave-owners, I think that Toulmin would say that it is not enough to consider the interests of the slaves only so far as to prevent the community from disintegrating into two communities. I think he really wants to invoke something like Sidgwick's Principle of Rational Benevolence ("that each man is morally bound to regard the good of his neighbour as much as his own") and while he does not wish to say, with Sidgwick, that this is a moral "intuition", he may be prepared to say that anyone who does not accept it is a lunatic or a psychopath,

is incapable of "doing ethics", just as some people are incapable of doing mathematics.

So far I have been arguing that, if moral principles are to be justified by certain formal criteria, these criteria themselves stand in need of justification; and that, if moral principles are to be justified by their success in harmonising desires, this is an ambiguous phrase which, if interpreted one way, leads to relativism, and, if interpreted another way, leads to objectivism. Toulmin, I have suggested, vacillates between these two interpretations. If I am right in all this, then we have not been shown that such questions as: How can morality be justified? or Is morality relative? are unnecessary and should not be asked. On the contrary, they seem to arise directly out of what Toulmin and Hare say.

But, it may be objected, philosophers who say of a given question that it cannot be asked are often believed (mistakenly) to be asking the question and answering it in the negative. Am I perhaps making this kind of mistake? We have been considering the question: Is one way of life (or moral code) ever better than another? The objectivist says "yes", the relativist says "no". Is Toulmin, after all, saying neither yes or no, but: "This question cannot be asked, because it has no real meaning?"

Now Toulmin does say, you will remember, that the question cannot arise in practice, except in those rare cases when a man is free to choose between remaining in our society or going to live, like Lawrence, as an Arab tribesman in the desert. And in this case it is a personal decision, and not a moral one. It cannot arise otherwise, because "there is no magic wand which will turn the English social system into a Muslim one overnight". But this is true only if we take "overnight" literally. A Kemal Ataturk, for example, or even a General MacArthur, may well have to decide whether to work for or against the westernisation of an eastern country. He can hardly avoid asking himself: Which way of life is the better? This is even more obviously true of the administrator of a colonial territory. It is sometimes even claimed that such mild social changes as those made by the recent Labour government in Britain have brought about, or at least started to bring about, a new way of life; and he would be a very short-sighted statesman indeed who did not take account of these long-term consequences. We can perhaps avoid the whole question by saying, with the Marxists, that the change from one way of life to another is always forced on us by economic or technological considerations. But I do not think that either Hare or Toulmin would wish to say this; and of

course there are objections to saying it. Of course the social reformer is never presented solely with the question : which way of life is the better ? He has to answer the question in a social setting ; he has to consider how the change can be brought about, what immediate steps are practicable, what their short-term effects will be, and so on. But this does not mean that the more fundamental question can never arise.

Toulmin, as we have seen, is inclined to say that, when the question is thus transformed into one of social reform within a given community, it is to be answered along utilitarian lines. This suggests that that way of life is the better which makes those who live it happier. It is true that this answer is not nearly as clear as it seems. For whether we are happier (*e.g.*) in a Christian or a Muslim society will depend largely on our existing attitudes, values, etc., and these will depend, at least in part, on the community we happen to have grown up in. This makes it plausible to say that the question : which is the better way of life ? can only yield the answer : Christians will be happier in the one, Muslims in the other. The greatest happiness principle, then, may itself lead to a limited relativism. Even so, it is still possible to ask such questions as : Does this way of life kindle aspirations and desires which it fails to satisfy ? Does it, for example, encourage every citizen to try to become a millionaire, a goal which can obviously be attained only by a few ? And this is the kind of question that social psychologists do tend to ask about a given way of life. In their terminology, it is the question whether a culture is " integrated ". There is, too, always the possibility that the members of one or other community just " do not know what they are missing ". But here, of course, we encounter the notorious ambiguities of the term " happiness ". Nevertheless, I think it can be said that, in so far as Toulmin regards the greatest happiness principle, or something like it, as the criterion by which moral principles are to be judged, he cannot escape both asking and answering the question : which of two ways of life is the better ?

This becomes even clearer if we take the phrase " way of life " as referring, not to what the anthropologist would call culture patterns, but to diverging moral codes within a given community. Here Toulmin would say that we decide between them by asking which of these two codes will tend to " minimise inconvenience ", " prevent avoidable suffering ", " harmonise desires ". And I think he recognises that this criterion, applied within a community, does involve both asking and answering the question : which way of life is the better ?

Now I have suggested that there are both objectivist and relativist strands in Toulmin's moral theory. So far I have been saying that his objectivism really does involve answering "yes" to our question. I am not so sure that the relativist strand involves answering "no" to our question. For it really is extremely difficult in practice to distinguish between saying: "No, one way of life is never better than any other" and saying: "This question cannot be asked." Let us consider, for example, what some social psychologists have said about divergent moral codes within a single community. Hadley Cantril [1] gives us the following example. There is a Mr. Green who takes as his basic beliefs such principles as these: "All people deserve an equal opportunity in life, but not all get it; wealth is not a sign of any inherent superiority; race or creed should not lessen one's consideration for others", etc., etc. Mr. Jenkins, on the other hand, has as his basic beliefs a very different set of principles: that one man can be intrinsically above others, that members of lower social classes or coloured races are ignorant, stupid, dirty, and do deserve less consideration than others, and so on.

On the basis of these "standards of judgment", Cantril goes on, Mr. Green forms a liberal, and Mr. Jenkins a conservative "frame of reference". And if either of them is asked, or asks himself, how he should behave to a particular Negro, or to Negroes in general, or how he ought to vote on a bill for removing civic disabilities on Negroes, he can answer the question only within his frame of reference, *i.e.* by referring it to his standards of judgment. This certainly has some resemblance to Toulmin's account of what is involved in moral reasoning. If we ask the question: why do Mr. Green and Mr. Jenkins hold these basic beliefs? Cantril will be inclined to say, like Toulmin in his Aristotelian Society paper, that this question admits only of a biographical answer. It is of course Cantril's business, as a psychologist, to give a biographical answer; to show how these divergent beliefs arise from differences in the upbringing of Green and Jenkins, and perhaps from deeper psychological differences of the kind which the psychiatrist explores. Perhaps I am wrong in supposing that Cantril himself would say that no other kind of answer is possible; but it is easy to see how such a view could be defended. For, it might be said, no one can ask which of these "frames of reference" is the better without referring the question to his own "frame of reference". We can do no more here than affirm our own basic beliefs: which may, of course, involve a good deal of thought, since we do not always

[1] Cantril, H., *The Psychology of Social Movements*, 1941, pp. 16-21.

know exactly what our own basic beliefs are, or at least have not always formulated them to ourselves. And there is, once again, certainly some resemblance here to Hare's contention that to affirm a moral principle is to make a personal decision ; that " you ought to tell him the truth " may be interpreted : " If you do not tell him the truth, you will be breaking a general ' ought '-principle to which I hereby subscribe."

Now this view, it may be argued, is not really opposed to objectivism at all. We have here, not alternative answers to the same question, but answers to very different questions, which arise at different levels. Of course the social reformer, like Kemal Ataturk, or even Mr. Attlee, will in fact ask himself, on occasion, which of two ways of life is the better, *i.e.* " which shall I work for " ? And of course he can only answer that question " within his own frame of reference ", *i.e.* by referring to certain basic principles which he happens to accept. But he cannot take this latter fact into account in grappling with the moral problem itself. He cannot say to himself : " Since my decision merely results from a certain basic principle which I have been conditioned to accept, and my opponent's decision results from a basic principle which he has been conditioned to accept, there is no real reason to suppose that I am right and he is wrong." For to say this is to accept and reject his own " basic principle " in the same breath. If he accepts it, he accepts it, *i.e.* he does believe that he is right and his opponent is wrong. It is obviously contradictory to take as one's basic *moral* principle : " no moral principle is better than any other." The truth is that the relativist contention is not a moral principle at all. To assert it is to go beyond the sphere in which moral reasoning applies. It is to adopt the point of view of the sociologist or the psychologist, perhaps even of the philosopher. But it is not from this point of view that one has to answer the question : what ought I to do in such-and-such circumstances ? And it is this question that one cannot get out of so long as one remains a man.

If this account of the relativist strand in Toulmin and Hare is accepted, then they must be acquitted of the charge of inconsistency, since their objectivism and their relativism arise at different levels. Moreover, they have succeeded in showing that the whole question between objectivism and relativism is a pseudo-question. But I do not think that this account can be accepted. I do not think that Toulmin would agree that his denunciation of Hitler is valid only at one level, and that there is another level of thought at which Hitler must be considered as

merely developing a way of life, about which the question : " Is this better or worse than any other way of life ? " cannot arise. It is I think clear from what has already been said that Toulmin would not count any possible pattern of behaviour as a way of life, but only a pattern of behaviour resting on principles which conform to the formal criteria we have been discussing. This would give us at most a limited relativism, and would make " way of life " a highly restrictive term. Moreover, this limited relativism would seem to rest on something very like an old-fashioned moral intuition. For we have seen that these formal criteria, and in particular the one that would exclude rules of privilege, cannot be derived either from an analysis of the way people behave, or even from an analysis of the way in which people think they ought to behave. Nor is it, again, as I think Hare assumes, a necessary consequence of the fact that men need to live in communities, the smooth running of which depend on the formation of habits, which have to be learned and taught.

Now if we refuse to count as a " way of life " any pattern of behaviour that does not conform to a particular moral principle, we cannot say that all moral principles are valid only within a particular way of life, and cannot be used to judge between one way of life and another. For we have already judged between two patterns of behaviour in deciding that one is and the other is not worthy to be called a way of life. On the other hand, if we count as a way of life any pattern of behaviour actually adopted or defended by any individual or community, then we cannot say that any moral principle must conform to the criteria which, I have suggested, Toulmin and Hare wish to lay down. I conclude, then, that they have not succeeded in showing us how objectivism and relativism may be reconciled. The possible patterns of behaviour are, I would suggest, much more varied than either Toulmin or Hare seem to realise.

Summarising, then :

1. I have suggested that the analogy between ethics and science breaks down. We cannot, simply by examining the way in which people behave or the reasons which they regard as justifying their behaviour, arrive at the concept of a way of life consisting of principles which conform to certain formal criteria, *viz.* principles which are consistently followed, universally applicable, not rules of privilege, and mutually consistent. This concept does stand in need of justification, since it is neither universally accepted in practice nor universally defended in theory ; and it is possible to use the words " good " and " ought " without implying such a concept.

2. Nor can we justify this concept by appealing to the function of moral principles, whether we take this to be making it possible for men to co-operate, or simply making it possible to learn and teach habits.

3. Toulmin, who says that it is the function of moral principles to enable men to co-operate in communities, interprets this criterion ambiguously, so as sometimes to mean whatever mode of harmonising desires will prevent the community from disintegrating and sometimes to mean the full utilitarian requirement of the greatest happiness of the greatest number. The first interpretation would give us relativism, the second objectivism. I think this ambiguity is concealed from Toulmin because he assumes that any way of life must conform to the formal criteria already mentioned, an assumption which he shares with Hare.

And I would conclude from these considerations that, whatever we may feel about the problem of induction, the problems of moral philosophy remain open for discussion.

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III.—QUALITY ORDERS

By W. C. Clement

In recent times a number of writers have held that it is possible to discover or to construct orders of qualities in which each quality of a given kind occupies a unique position which is determined by its relation of likeness to other qualities of that kind. This is sometimes expressed by saying that qualities can be arranged in series which are determined by the natures of the qualities themselves; that is to say, that there are *natural* orders of qualities, just as there is a natural order of numbers.

In this paper I shall try to show that the reasons which have been given in support of this claim are inadequate, and that it is unlikely, although not impossible in principle, that quality orders of this type can either be discovered or constructed. The paper is divided into three parts. In Part I the contexts in which the claim arises are briefly described. Part II is devoted to the analysis of the claim. Specific topics to be dealt with in this part are serial orders, what I shall call "perceptual orders", natural orders, and complete perceptual orders. In Part III specific criticisms are made of the methods of constructing quality orders which have been proposed by W.E. Johnson, Moritz Schlick and, most recently, by Nelson Goodman.

I. *Introduction*

One of the incentives which often leads to the attempt to find or construct quality orders is well expressed by Poincaré when he writes, "Sensations are therefore intransmissible, or rather that all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations."[1] Since the relations between qualities are transmissible in a sense in which the qualities themselves are not, communication requires the presupposition that there is a natural order of qualities between which there are certain fixed relations. It is in virtue of these relations that communication is possible. Few writers, however, have attempted the elaborate task of specifying in detail how the required natural order of qualities is to be determined.

W. E. Johnson was the first to attempt to give specific

[1] *The Value of Science*, The Science Press, N.Y., 1907, p. 136.

instructions for the determination of quality orders. He pointed out that the *determinates* under a given *determinable* can be compared with each other, and that the differences between sets of two determinates can also be compared with each other. For example, the difference between red and yellow is greater than the difference between red and orange. Since such comparisons are possible, Johnson concluded, "... the several determinates are to be conceived as necessarily assuming a certain serial order, which develops from the idea of what may be called 'adjectival betweenness.'"[1]

Thus Johnson reasons from the possibility of comparing quality differences to the fact that there exists a unique series of qualities (determinates under a given determinable) which has its order *determined* by the natures of the qualities themselves. There is, therefore, an order of qualities which is not arbitrary, but which is *natural*. Johnson holds that the recognition of quality orders provides the answer to the question whether relations are external or internal.

Schlick argues in a similar vein. He holds that the relations between qualities, like the relations between numbers, are internal relations. By its very nature, a quality occupies a definite position in a range of qualities, and this position is determined by the relations of similarity and dissimilarity to other qualities of the same type.[2] This claim is of central importance to Schlick's major thesis in the essay "Form and Content": to the effect that the *content* of the world, or what Poincaré called "pure quality", cannot be communicated.

The most detailed examination of the concept of quality orders has recently been made by Nelson Goodman in *The Structure of Appearance*. One of his objectives is "to construct, for each category of qualia, a map that will assign to each quale in the category a unique position and that will represent relative likeness of qualia by relative nearness in position".[3] To construct this map it is necessary to define elementary ordering predicates in terms of a chosen primitive. The ordering predicate used by Goodman is *matching*. Later he makes a transition from *matching* to *betweenness* by means of a rule of order, whose purpose is to provide a means of constructing an order of qualities in terms of matching. Goodman proposes to use the quality orders he has thus constructed for a twofold purpose: to

[1] *Logic* (Part I), Cambridge, 1921, pp. 181-182.

[2] In the essay "Form and Content" printed in *Gesammelte Aufsaetze*, Vienna, 1938, p. 162.

[3] *The Structure of Appearance*, Harvard University Press, 1951, p. 218.

develop a systematic nomenclature of qualia in a category,[1] and to provide the foundation for a calculus of shape and measure.[2]

It is clear from this brief review that the claim that there is a natural order of qualities figures in some important philosophical doctrines. If the thesis of this paper is correct, then these doctrines, if true, will have to be supported by other arguments than those which have been used.

II. *Analysis*

While this paper deals with the possibility of quality orders in general, in the following discussion I shall only consider the possibility of constructing an order in one dimension, so to speak, of a particular type of quality. This restriction will simplify the discussion and at the same time leave us with an order of qualities which is almost exclusively employed by the writers on this subject. I shall restrict the discussion to the possibility of constructing an order of hues. The other dimensions of colour, saturation and intensity, need not be considered. Evidently if there is a difficulty in ordering hues, then there is a difficulty in ordering colours; and if there is a difficulty in ordering colours, then there is likely to be a corresponding difficulty in ordering other types of qualities.

In this connexion it is very important to recognise that the issue to be considered has nothing whatever to do with the *causes* of colour, or with any scientific theory of colour. In philosophical contexts all qualities are on an equal footing, however they may be regarded in scientific circles. Thus we shall be concerned with the possibility of ordering all hues, non-spectral as well as spectral, in an order in which each hue will occupy a definite position determined by its own nature.

It is also important to recognise that we cannot assume the goal of the inquiry to be the construction of a particular order of hues, such as is found in the spectrum or in the colour cone. Even if the constructed order were to coincide with either of these, its significance would not lie in this coincidence, but only in the fact that it had been constructed by considering only the natures of the hues themselves.

In the following sections I shall consider the various issues and possibilities which arise in the attempt to construct quality orders. In Section A the possibility of finding a serial relation which will order hues in a serial order is discussed. In Section B are outlined the basic features of what I shall call "perceptual

[1] *The Structure of Appearance*, p. 218. See also p. 278. [2] *Ibid.* p. xv.

orders ". In Section C an attempt is made to show that there are no natural perceptual orders, and finally, in Section D the possibility of *complete* perceptual orders is considered.

A. *Serial Orders*

A relation which is to order a set of terms must be such that it will be possible to say of any two terms that one *precedes* and the other *follows*. Relations of this type are called "serial" relations, and they are said to "generate" serial orders. A simple example of a serial order is the set of numbers which is generated by the serial relation *successor of*. Other examples are sets of points along a line generated by the relation *to the right of*, or sets of moments in time generated by the relation *later than*.

Consider now the set of all distinguishable hues. How shall they be arranged in a serial order? The relation of similarity (or dissimilarity) cannot be used to generate a serial order of hues since it is not a serial relation: terms related by similarity are grouped in sets which are determined by the similarity of various terms to a certain specified number of terms. If hues are to be arranged in a serial order, then some relation other than similarity will have to be used. What other relation can be used for this purpose?

It may be suggested that the relation *between* could be used to generate a serial order of hues. This possibility will be found to be ruled out when we come to discuss perceptual orders. It is, however, to be noted in this connexion that questions asking why one hue is to be considered to come between two other hues cannot be answered in terms of the similarities between hues, since similarity is not a serial relation.

Is there a serial relation which generates a serial order of hues? No relation which will serve this purpose has ever been cited by writers on this subject. Of course it does not follow that it is inconceivable that such a relation could be found; however, in the following sections I shall consider a fundamental characteristic of hues which suggests that the discovery of the required relation is very unlikely.

B. *Perceptual Orders*

It may be asked whether the foregoing emphasis upon serial relations does not overlook an important insight into the nature of hues. Is it not possible that hues form some kind of order, even if it should turn out to be an order which is different from a serial order?

Common sense considerations might well suggest that qualities fall into some kind of order. Orange, for example, is generally thought to come between red and yellow, rather than that yellow comes between red and orange. If this relation, call it "qualitative betweenness", obtains between these three hues, why should it not also relate all other hues? Since, however, the term "between" is not used in its spatial sense, the only remaining problem would seem to be to formulate rules in accordance with which judgments, such as those concerning the relation between red, yellow and orange, are made. It would then have to be shown that the application of these rules to all hues would result in the contemplated order of hues.

A clue to the rule required is given by the example of the spectral order of hues which is generally used. The important features of this order are (1) that there is a gradual transition from one hue to another and (2) that the hues which are most similar are spatially contiguous. I shall assume that these *are* the important characteristics; and they can be summed up by saying that the series of hues is *smooth*. Series which are smooth I shall call "perceptual orders". They will be found to have the following properties:

(A) Adjacent terms are indistinguishable.
(B) No indistinguishable terms have between them a term which is distinguishable from either of them.[1]

It is important that the terminology used in stating these properties be understood in the precise sense in which they are intended. The *term* of a perceptual order is a particular hue, such as red, blue, purple or beige. Two terms are *adjacent* to each other when they are either spatially contiguous or follow each other temporally. In the spectrum adjacent hues are spatially contiguous; however, two hues which are projected on to a screen successively are also adjacent. (Sounds, for example, can only be adjacent in the temporal sense.) A term b of a perceptual order is between a and c (where a and c are distinguishable) when it is one of a set of terms which makes the transition from a to c smooth.

Can we be sure that writers on this subject have had in mind the type of order which is outlined above? I think we can, since the quality orders they speak of meet these conditions. Most important, however, is the fact that (A) and (B) are

[1] Property (B) would have to be stated in a slightly different form for circular perceptual orders. The possibility of circular perceptual orders does not affect the following discussion.

necessary conditions for an order of qualities which is smooth and which represents relative likeness by relative nearness in position.

C. *Natural Orders of Hues*

I shall now examine the view that there is a *natural* of hues ; that is, that it is *of the very nature of* one hue to range between two specific other hues. I shall assume that if there were a natural order of hues, some statements of the form " x is between y and z ", where x, y and z are hues, would be false. For example, if it were of the very nature of orange to range between red and yellow, then it would be false to say that orange ranged between brown and yellow, or between green and blue.

There are a number of common sense considerations which lead to the conclusion that there is no natural order of hues in the above sense. Consider again the familiar arrangement of hues in the spectrum.[1] Suppose we wish to add hues into this order which are not already contained in it. In the case of purple there seems to be no difficulty, since it is usually placed between red and violet. Where, however, are the other non-spectral hues to be placed ? It is clear that no matter where in the original arrangement it is placed, it will be necessary to introduce intermediate hues to make the resultant order smooth. Is it significant, therefore, to say that brown, for example, has a *natural* place, determined by its likeness to other hues, in the system of hues ? Is it of the very nature of brown to be placed (or found ?) between blue and green rather than between red and purple ? The answer is clearly negative, since brown can be placed between any hues whatever in a perceptual order.

Now the foregoing considerations apply to every hue, be it spectral or non-spectral. One reason why, for example, orange may be thought to range naturally between red and yellow is that this is the place in which it is found in the spectrum. From the point of view of philosophy, however, this is a merely factual arrangement of hues ; it being quite conceivable that orange might have been found to range between, say, indigo and blue. There is nothing self-contradictory in this supposition. Another reason why (to take the same example) orange might be thought to range naturally between red and yellow is that the mixture of red and yellow lights produces an orange colour. This again, however, is a purely contingent fact which can have nothing to do with the philosophical issue of quality orders. (In this

[1] We are here concerned with the arrangement of hues, not with the fact that it has been produced in a particular way.

connexion it will also be helpful to recall that, for the most part, the results in mixing coloured lights are different from the results of mixing pigments.)

The conclusion of these considerations is that any hue can range between any others. This conclusion can be aptly demonstrated with a simple thought experiment.

Consider an experimental arrangement which provides a means of superimposing on a screen different hues in different degrees of intensity. Consider an arrangement with three projectors which permit the projection of three different hues, which we shall call "reference hues". By varying the intensities of the reference hues, the intensity of the resultant hue on the screen can be kept constant. Consider an arrangement in which the reference hues are red, brown and green. The experiment is then conducted as follows: first red light at the full intensity is projected onto the screen. Then, as its intensity is decreased, the intensity of brown is increased until the full intensity of brown is projected. The brown is then diminished while the green is increased, until the reference hue green is projected onto the screen at full intensity. In this way we shall have passed from red to green through brown in a way which makes the resultant order a perceptual order.

By means of this experiment we can see that perceptual orders can be obtained from any reference hues whatever; from red, green, blue, vermilion, brown, or from the many unnamed hues which result from the superposition of the named hues. Since any perceptual order is as *natural* as any other, whatever reason could be given for saying that x ranged between y and z would also hold for saying that x ranged between u and v. Thus the claims to the effect that certain hues must, of their very nature, range between certain other specified hues are unfounded.

The foregoing result can be summarised in the form of a diagram. Consider a certain hue A to be represented by a point which lies at the centre of a circle. The points throughout the circle and along its periphery represent other hues which are arranged to make spatially contiguous hues indistinguishable. Let us label the hues at the twelve, nine, six, and three o'clock positions B, C, D, and E, respectively. The hue A will be seen to be intermediate between a large number of hues, such as B and D, and C and E. Since the "path" between hues need not be straight lines, A can also be considered to lie between B and C, or E and B. In fact, all paths, except those which turn back upon themselves to the extent that different portions of the series will contain indistinguishable hues, will result in perceptual orders.

D. *Complete Perceptual Orders*

The conclusion of the foregoing section will help us to see some difficulties in the claim that there is a *complete* perceptual order of any quality. By a " complete perceptual order of hues ", for example, I mean a perceptual order which contains *all* distinguishable hues, spectral and nonspectral. Any attempt to construct orders of qualities which will serve the philosophical purposes for which they are intended will have to take into account all of the distinguishable qualities of the given kind. Now all the writers on this subject have dealt with complete perceptual orders.[1]

I shall now try to show that a certain method by means of which it might be thought possible to construct complete perceptual orders will not work. However, I do not believe it is possible to show that it is in principle impossible to construct complete perceptual orders: any method which may be suggested, therefore, will have to be examined on its own merits.

Commonsense considerations might well suggest the possibility of complete perceptual orders. Consider again the case of hues. It is clear that the number of distinguishable hues, though large, is finite, and since any required intermediate hues can always be provided, it would appear to follow immediately that complete perceptual orders can be constructed. However, it is unlikely that such an order would be a complete perceptual order, and for the following reason.

Consider again a specific arrangement of hues, such as is found in the spectrum. Suppose that we wished to add to this perceptual order the hues which would make the resultant order a complete perceptual order. It will then be seen that, initially at any rate, it is not the case that it makes no difference where in the original order the new hues are to be placed; for their positions within the original order will determine the kinds of intermediate hues which have to be supplied to make the resultant order smooth. Thus the very fact that a certain sequence of hues is chosen seems to exclude the appearance of certain hues from the perceptual order. A brown-orange may not appear in a perceptual order in which the sequence of hues is from green to

[1] Referring to colours, Johnson writes that, " the *whole series* has its order directly determined by the nature of the adjectives themselves " (*Logic*, p. 182, italics mine). Schlick writes, " In this way every quality . . . is interconnected with *all* others by internal relations which determine its place in *the system of colours* " (*Gesammelte Aufsaetze*, p. 162, italics mine). And Goodman states that the problem with which he is concerned is " to construct for each *category of qualia* " a quality order (*The Structure of Appearance*, p. 218, italics mine).

brown to blue, and a brown-green may not appear in a perceptual order in which the sequence of hues is from orange to brown to red. Whatever sequence of hues is decided upon, some distinguishable hues would seem to be excluded by the choice of that sequence.

It appears that the only way in which a series of hues could contain all distinguishable hues would be if indistinguishable hues were to appear in separate sections of the series. In the diagram which we considered before, this would amount to making the transition from A to all of the hues represented on the periphery of the circle by means of a spiral path. In this way all hues which are intermediate between A and the hues represented on the periphery of the circle would be included in a smooth series of hues. Such a series, however, would not be a perceptual order, since indistinguishable hues would appear in separate sections of the series.

Are there other methods which could be used to construct complete perceptual orders? In the preceding discussion we have only considered one method : it has not been shown, however, that it is in principle impossible to construct complete perceptual orders. It seems to me unlikely that a proof to this effect can be given. Nevertheless the preceding discussion has brought out a fundamental difficulty which would be encountered in any attempt to construct complete perceptual orders. It is not easy to see how this difficulty could be overcome ; at any rate no writer on the subject has taken it into account.

The difficulty mentioned above arises only in connexion with the attempt to construct complete perceptual orders : it is not encountered in the construction of perceptual orders in which indistinguishable hues are allowed to appear in separate sections of the series. It may, therefore, be asked why property (B) should be specified at all. To say that unless a series met the requirements of (B) it would not be a " real " perceptual order would be trivial. There is nevertheless a good reason why (B) must be stipulated, for if indistinguishable hues are allowed to appear in separate sections of the series, then assertions to the effect that x ranges between y and z would lose their significance since it would be equally true that x ranged between u and v.

E. *General Conclusions*

The results of the foregoing discussion can be summarised as follows :

1. There are a large number of perceptual orders which do not include all distinguishable hues.

2. There are a large number of smooth arrangements of hues which contain all distinguishable hues. These arrangements are not likely to be perceptual orders, however, since indistinguishable hues will probably appear in separate sections of the series.

3. No method for constructing complete perceptual orders has been found. Now even if a method were found, it would still have to be shown that a *unique* or *natural* arrangement of hues would result. Only under these conditions would it be significant to say that it is of the very nature of a certain hue to range between two other specific hues.

In the preceding account I have dealt only with hues. Now it follows that if the conclusions about perceptual orders of hues are correct, then they will also apply to colours; and if they apply to colours, then they are also likely to apply to other types of qualities. The conclusion can therefore be stated in general terms as follows: *It has not been shown to be possible to construct complete perceptual orders of qualities in which each quality of a certain type occupies a unique position determined by its relation of likeness to other qualities of that type.*

It may be asked if it might not be possible to construct complete perceptual orders of qualities in terms of some other principle than the one considered in this paper. It might be rash to assume that the failure of one attempt forebodes the failures of any other attempts which might be made. Yet all attempts to construct quality orders up to the present time have been attempts to locate the position of each quality in accordance with its relation of likeness to other qualities within the order. This fact may well leave us with the reasonable doubt that complete perceptual orders of qualities in which each quality of a given type has a *unique* position, determined by the natures of the qualities themselves, can be found or constructed.

III. *Criticisms and Implications*

In the first part of this paper I referred to writers who have tried to outline methods to be used in the construction or discovery of natural quality orders. Having completed our analysis, it will now be interesting to note where the difficulties with the proposed methods lie. It will also be of interest to note how their inability to construct such orders affects the more general philosophical positions which these writers wish to maintain. In the remainder of this paper I shall take up these two topics in connexion with the arguments presented by W. E. Johnson, Moritz Schlick, and Nelson Goodman.

A. *W. E. Johnson*

In explaining the distinction between determinables and determinates, Johnson points out that the *differences* between the determinates under a given determinable can be compared with each other. He infers from the fact that such comparisons are possible that the determinates under a given determinable necessarily assume a certain serial order which "develops from the idea of what may be called 'adjectival betweenness'".[1] "Between" is here used in the metaphorical sense derived from spatial relations: "Thus if *b* is qualitatively between *a* and *c*, and *c* qualitatively between *b* and *d*, and so on, the whole series has its order directly determined by the nature of the adjectives themselves."[2] This argument consists of two parts which we shall consider separately.

(i) Does it follow from the fact that *differences* between hues can be compared that hues necessarily assume a certain serial order; an order determined by the natures of the hues themselves? In order to answer this question, consider an experimental arrangement in which two of the reference hues are red and blue, and another arrangement in which two of the reference hues are red and green. How shall we determine whether the difference between red and blue is greater than the difference between red and green? To answer this question we cannot make use of a *de facto* order of hues, like the spectrum, since the resultant order is to reflect the relative likenesses of hues, and is not to be a reconstruction of a given order. Under these circumstances the most likely way of deciding the question would be by making use of the following convention: the assertion, "the differences between two hues *a* and *b* is greater than the difference between *c* and *d*" is to be interpreted to mean that the minimum number of steps, each of which is indistinguishable from those adjacent to it, required to pass from *a* to *b* is greater than the number of steps required to pass from *a* to *d*. The use of this convention provides us with unambiguous meanings for statements about differences between hues.

Let us assume that all the differences between hues have been determined in the above manner. Would this datum provide us with the evidence required to show that hues "are to be conceived as necessarily assuming a certain serial order?" It would not; for any given hue would be "surrounded", so to speak, by a large number of distinguishable hues, each of which is *equally* different from the given hue. Thus, for example, it may well be that blue and green, according to the convention suggested,

[1] *Logic*, p. 182. [2] *Ibid.* p. 182.

would turn out to be equally different from red, or that red and violet would be equally different from beige. In short, any hue can be approached from many different qualitative " directions ", and along each of these directions there will be distinguishable hues which are equally different from the given hue. When this situation obtains, it is not possible to construct a unique perceptual order of hues.

(ii) Let us now consider the related argument in which the concept of adjectival betweenness is used. Does it follow from the fact that if *b* is qualitatively between *a* and *c*, and *c* between *b* and *d*, and so on, that the whole series has its order directly determined by *the nature of the adjectives themselves ?*

The previous analysis provides the necessary qualifications for answering this somewhat misleading question. Consider again the case of hues. It has been shown that any hue can appear between any other two hues, and that, therefore, any statement to the effect that *b* is between *a* and *c* is a statement about a *particular* perceptual order. We can now see why the above question is misleading ; for it follows that, if the terms are related in the manner described, then these terms can be members of a set of terms constituting a perceptual order of hues. But this order will simply be one of a large number of possible perceptual orders. Johnson has not succeeded in constructing quality orders in which each quality has a unique position determined by its relation of likeness to other qualities of the same kind: instead, he has merely presupposed a particular quality order and made statements about it. It seems likely that any rules specifying how a perceptual order might be set up will inevitably be involved in circularity, since in order to specify the means for setting up an order, the rules themselves presuppose some order which is considered to be the natural order.

I shall now consider briefly the general philosophical implications of Johnson's failure to provide a satisfactory method of constructing quality orders. According to Johnson, quality orders have an important bearing upon the problem of internal relations. One school holds that all relations are external ; the other school holds that they are all internal. Johnson's solution to the problem is that all relations between adjectives (*i.e.* qualities) as such are internal, while relations between existents as such are external.[1] Now if this were true, then such statements as " *b* ranges between *a* and *c* " would be true in virtue of the very nature of the qualities *a*, *b*, and *c*. However statements of this type have been shown to be synthetic and, if true at all,

[1] *Logic*, p. 250.

they are only true in virtue of the existence of a particular order of qualities. Thus Johnson's solution to the problem of internal relations is invalid.

B. *Moritz Schlick*

In the essay, "Form and Content",[1] Schlick introduces the subject of quality orders in connexion with his discussion of the incommunicability of what he calls "content". Since we can only communicate the *structures* of facts, and since, by hypothesis, we do communicate about the colours of things, it follows that colours must have a structure. In saying that colours have a structure Schlick means that the relations which hold between the elements of the system of colours are *internal relations*. By an "internal relation" he means a relation that is "necessarily implied by the very nature of the terms".[2] All relations between numbers, for example, are internal relations.

> Similarly, it is not an accidental property of green to range between yellow and blue, but it is essential for green to be related to blue and yellow in this particular way, and a colour which were not so related to them could not possibly be called 'green' unless we decide to give this word an entirely new meaning. In this way every quality (for instance, the qualities of sensation; sound, smell, heat, etc., as well as colour) is interconnected with all others by internal relations which determines its place in the system of qualities.[3]

Schlick's claim that there is a natural system of qualities rests upon the validity of such statements as, "it is essential for green to range between blue and yellow." However, he does not present any arguments in support of this position, but seems to assume that statements of this type are self-evident. We have shown that this is far from being the case. Like Johnson, Schlick does not offer a method of constructing a natural system of qualities, but merely presupposes the existence of such an order.

The implications of these criticisms for Schlick's major philosophical thesis in the essay "Form and Content" are crucial. This thesis is to the effect that the *content* of the world cannot be communicated, and that we can only communicate the *structures* of the facts which compose the world. Thus, if we communicate about qualities, then qualities must have a structure; that is to say, they must stand in internal relations. And they will stand in internal relations only if they form a natural system, like the system of numbers. Since, however, they have not been shown to constitute a natural system, Schlick's account of communication is inadequate.

[1] Included in *Gesammelte Aufsaetze*, Vienna, 1938.
[2] *Ibid.* p. 162. [3] *Ibid.* p. 162.

C. *Nelson Goodman*

Goodman begins his discussion of this subject by selecting an ordering predicate which he calls "matching". This relation

> . . . distinguishes between every two distinct qualia to which it applies, through the fact that there is always some quale that matches one but not the other of two distinct qualia: and thus it assigns a distinct position to each quale in a category, whether that category is 'unidimensional' or 'multidimensional'.[1]

This statement is not quite correct, however, since the relation *matching* is symmetrical, and therefore cannot assign a distinct position of each quale in a category. Goodman, in effect, recognises this when he later makes the transition from matching to betweenness by means of "a rule of order". The rule states *that the span between any two matching qualia is less than the span between any two nonmatching qualia.*[2]

But does the rule provide a means of ordering qualities? This it does not do, and Goodman seems to have been led to think that it does as a result of a misleading discussion which precedes the formulation of the rule.

Goodman asks how we shall define, in terms of matching, such explicitly ordering predicates as "is between" and "is next to". He answers this question by explaining how the term "between" will be used.

> A simple case of transition from matching to betweenness is provided when, from the fact a quale *b* matches two others *a* and *c* that do not match each other, we conclude that *b* is between *a* and *c*. The considerations supporting this transition seem to be more or less as follows: that two matching qualia are more alike than two nonmatching qualia; that relative likeness represents relative nearness; and that, according to one usage of 'between' *b* is between *a* and *c* if *b* is nearer to *a* and to *c* than *a* and *c* are to each other.[3]

The above passage raises a number of issues. (i) First, there can be no objection to a convention which allows us to say that a quale *b* is between *a* and *c* when it matches *a* and *c* and *a* and *c* do not match each other. It is, however, misleading to overlook the fact that there can be a large number of qualia which do not match, but which also stand in the same relation to *a* and *c* as does *b*. Now all the qualia comparable to *b* in relation to *a* and *c*, namely, b', b'', b''', . . . b^n, cannot be ordered in the way prescribed for the qualia *a*, *b*, and *c*. Therefore the assertion that *b* is between *a* and *c* is misleading in that it suggests that there is only *one* quale between the two others. If, on the other hand, this suggestion is not intended, some other means will have

[1] *The Structure of Appearance*, p. 224. [2] *Ibid.* p. 241. [3] *Ibid.* pp. 240-241.

to be provided for ordering all of the terms which stand in the same relation to two qualia as *b* does to *a* and *c*.

(ii) Goodman supports the transition from matching to betweenness by pointing out (*a*) that two matching qualia are more alike than two nonmatching qualia, and (*b*) that *relative likeness represents relative nearness*, and that according to one usage of "between", *b* is between *a* and *c* if *b* is nearer to *a* and to *c* than *a* and *c* are to each other. Now (*a*) merely reflects the way in which the term "matching" is to be used. However, (*b*) is involved in the same difficulty which was discussed in connexion with Johnson's views: instead of the problem of comparing *differences between determinates*, we now have the problem of discovering the *relative likeness* (or nearness) *of qualia*.

In order to bring out the implications of these criticisms on Goodman's general philosophical position it is necessary to consider the two purposes which quality orders were intended to serve. The first purpose is that "by defining a set of co-ordinates, one may develop a systematic nomenclature for the qualia in the category".[1] However, any attempt to identify a certain quale by specifying that it lies between two others, or that it is near another, will fail because it will not single out a definite quale. Therefore this procedure does not make possible a systematic nomenclature for the qualia in a given category.

A full discussion of the second purpose which the construction of quality orders is to serve would require an evaluation of the entire method of construction which Goodman utilises. Here, however, I must restrict myself to a brief statement of the immediate consequences of the above criticisms. The construc- of quality orders is to provide the foundation for a calculus of shape and measure.[2] Goodman rejects the possibility that size and shape terms are to be interpreted as naming "individuals" in his system: "They are denied the status of names of atoms or other individuals in our system because, so to speak, there are important ways in which they do not behave like other terms that are taken as naming individuals in the system."[3] The crucial considerations are expressed as follows:

> The specific qualia that an individual contains fix its size and shape with respect to each category of qualia. If we can count and are familiar with the order of qualia in a given category, then we can determine the size and shape of an individual *x* in that respect if we know exactly what qualia of that kind are contained in *x*. For the arrangement of a given set of qualia is not variable. Qualia cannot be literally 'moved around'; each has—we might almost say *is*—a fixed position in the array of the category to which it belongs.[4]

[1] *The Structure of Appearance*, p. 218. See also p. 278.
[2] *Ibid.* p. xv. [3] *Ibid.* p. 204. [4] *Ibid.* pp. 211-212.

It is not entirely clear whether Goodman intends to assert that the sizes and shapes of individuals can *only* be determined by having available orders of the qualia contained in the individual. It would seem that one could know what qualia are contained in an individual without having constructed such orders. And if this is possible then it is no longer clear why the determination of the shapes and sizes of individuals would depend upon the construction of quality orders. At any rate, if there were such a dependence, then the shapes and sizes of individuals could not be determined in Goodman's system, since the required types of quality orders have not been constructed.

IV.—THREE PROBLEMS ABOUT OTHER MINDS

By W. W. Mellor

Part I of this paper is a general discussion of the way we use expressions like " . . . is in pain " in everyday language: its principal object is to throw light on what is meant by saying " X's pain is like Y's ", and the sense in which " I feel pain " can mean the same to different speakers. Part II is concerned with *a priori* objections to the significance of " X feels pain ", non-behaviouristically understood: its thesis is that such objections can be answered by a decision about the use of words like " meaningful "; and that certain criteria for a reasonable, though not for a *right*, decision can be laid down. Part III deals with the question " Can the argument from analogy yield a probable conclusion? "; I try to show that this problem is analogous to, though not a special case of, the Problem of Induction. Each of the earlier parts of the paper is an attempt to eliminate difficulties which might complicate the discussion of the later topics; but otherwise the parts are independent.

I

There are no simple necessary connexions between " Jones is in pain " and statements of the kinds which could be given as reasons, by persons other than Jones, for believing that Jones is in pain. " Jones is in pain " does not entail any disjunction, nor is it entailed by any conjunction, of statements such as " Jones is groaning ", " Jones has a swollen jaw ", " Jones says it hurts ", " Jones is being drilled by the dentist ". For among the sentences which express the statement that Jones is in pain is " I am in pain ", said by Jones; and Jones can easily imagine a situation in which he feels pain although his pain is in no way manifest to others, or a situation in which he does not feel pain although everything about him would suggest to others that he did. For the experience of being in pain is unlike the experiences of groaning or hearing oneself groan, discovering that one's jaw is swollen, or observing what is being done, even to oneself, by the dentist. To imagine any of these things is not to imagine pain.

But there is some more complex logical relation between pain and its causes and manifestations. For such things as painful

situations, symptoms of pain, and remedies for pain are inextricably involved in all processes of teaching or explaining the meaning of the word. To teach someone the meaning of " pain " we could prick him and say " Now you are in pain ". Or we can explain its meaning by saying things like " It's the sort of feeling you get when you are kicked, pricked, burnt or drilled by the dentist. It's the sort of feeling you often have when your jaw is swollen or your ankle sprained. It's the sort of feeling that tends to make you groan or wince. It's the sort of feeling that tends to go if you rub the place where you feel it, apply certain ointments or take drugs like aspirin and opium." Such explanations will be successful in proportion as the listener has experience of the kinds of situations, manifestations and remedies mentioned, and can identify a feeling characteristic of most, if not all, of them. But are the various things mentioned in our explanations merely useful pointers, contingently connected with what we want our pupil to identify ? Or are they part of what he must learn if the lesson is to succeed ?

Now, the logic of " I am in pain " is comparable to that of " qualified-claim " statements of perception. To say " I feel pain " is not indeed to say " I feel as though I were being drilled by the dentist " ; but it is to say that I feel as I usually do feel in a certain range of situations associated with certain types of behaviour, bodily condition and characteristic remedies. Imagine a man who for a long time has experienced neither pain nor itches, who now experiences a mild degree of a feeling which he describes by saying " I feel a pain ". Suppose that shortly afterwards he goes to a dentist and experiences a feeling which makes him want to scream ; he is also subjected to red-hot pokers, pins and rubber truncheons, and experiences feelings similar to this in each case. At about the same time he is tickled with a feather and experiences a feeling which makes him want to laugh ; under the influence of fleas and itching-powders he experiences further feelings of the same kind. Then suppose he says " My first feeling was not like the kind of feeling that made me want to scream ; it was like the feeling which made me want to laugh ". Surely it follows that he was wrong in saying " I feel a pain " when he first said this ; that what he felt was a tickle and not a pain ?

But a puzzle can arise out of this. We can distinguish two types of statements ; the first may be illustrated by " I feel as though I were being drilled by the dentist ", " It sounds to me like the call of the nightingale ", " It looks to me like a pillarbox ". The second can be illustrated by " I feel a pain " or " This looks

red to me". Now the first kind of statement, though not the second, admits of a certain kind of factual mistake, though not a mistake which is capable of being corrected by others than the speaker. The man who says "I feel as though I were being drilled by the dentist" may have forgotten what it feels like to be drilled by a dentist. The man who says "It sounds to me like a nightingale" may have confused the cry of the nightingale with that of the corncrake. The man who says "It looks to me the colour of a pillarbox" may hold the mistaken belief that pillar-boxes are painted green. In each of these cases, the mistake is not linguistic: it makes sense to talk of the speaker's checking his statement by seeing what the relevant object does in fact feel, sound or look like to him. He knows the meaning of his statement if he knows the proper procedure for checking it, in this sense. The trouble is only that when he does check it, he gets a surprise.

But we cannot say that the man who confuses pains with tickles is mistaken in quite this way. It is queer to talk of "checking" "I feel pain", in a way in which it is not queer to talk of "checking" "I feel as I would if I were being drilled by the dentist". It is certainly not part of the meaning of "I feel pain" that it should be checked with regard to dentists' drills, red-hot pokers or anything else; whereas it is part of the meaning of "I feel as though I were being drilled" that the appropriate tests involve drills. All that can be *deduced* from "I feel pain" is "I feel as I would in a situation which caused me pain". Forgetting what situations which cause me pain feel like is forgetting what pain feels like, forgetting the meaning of "pain"; it is clearly unlike forgetting what dentists' drills feel like.

This is disconcerting: for it seems easy to conclude from it that "pain" is a word which stands for a certain quality of sensations, in a sense which makes it hard to see different speakers can attach the same meaning to "pain" or how we can be sure that X's pains resemble Y's—indeed, how we can attach a meaning to the statements that they do or do not resemble them. To see what is wrong with this, we may first examine the comparable behaviour of the expression "this looks red to me".

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To say "This now looks red to me" is to say "This now looks to me, in point of colour, as a red object would look to me under normal conditions and provided I were a normal observer", a "normal observer" being one to whom red things look alike and unlike things of other colours. This is unlike saying "This now

looks to me as would a pillar-box " : to forget how pillar-boxes normally look to me is to forget a fact, but to forget how red objects look to me would be to forget the meaning of " red ". So we might say " Knowing the meaning of ' red ' involves knowing what red objects look like to me ". But it does not follow that " red " names a quality of sensations, or that X can or must describe how red objects look to him in some way which does not itself involve a reference to objects. For " I know what red objects look like to me " is simply a way of saying " I can recognise red objects ", " I can discriminate red objects correctly ". To know the meaning of " red " is to be able to apply the word " red " to the right objects, just by looking at them ; and for X to do this, it is necessary and sufficient that red objects should look alike to him, and unlike blue, green or yellow things, and that he should call them " red " because of this resemblance.

It follows that the word " red " need not, and normally does not have different meanings for different speakers. For X and Y are agreed in their use of " red ", to the extent that they call the same things " red ". This is not to say that to know what others mean by " red " is simply to know what objects they call " red " : for to be able to discriminate objects in the same way as others is not the same as to notice what discriminations they make.

It follows also that one person's use of " This looks red to me " can be explained to and understood by another person. For if X can understand Y's use of " red object ", he can also understand what Y means by " this looks to me now as a red object would normally look to me ". Somebody might object that this is not sufficient to ensure that X *fully* understands Y's use of " this looks red to me " ; for X does not know what red objects normally look like to Y. This can be illustrated by an analogy : if someone says to X " This looks like an autogyro ", there is a sense in which X can understand if he only knows, from hearsay, what autogyros are ; but to understand fully he must also know what autogyros look like. But the analogy shows what is wrong with the objection : for clearly the sense in which X may not know what autogyros look like is unlike the " sense " in which he necessarily does not know what red objects look like to Y. His ignorance of what autogyros look like involves his not knowing what autogyros would look like to him ; but it is absurd to talk of his knowing or not knowing " what red objects look like to Y " would look like to him. X does, of course, know that red things look red to Y, if Y is normal. But he does not know

what they look like to Y in the sense in which he knows what they look like to himself. "X knows what red things look like to him" means "X can recognise red things": we obviously cannot find a parallel translation for "X knows what red things look like to Y". But this is not a good reason for saying that X's understanding of "This looks red to Y" is incomplete.

Likewise there is no problem whether or not red objects look the same to different persons. For if X and Y both say of a certain object "This looks red to me", and are agreed in their use of "This looks red to . . .", it follows that the object looks the same to both. Sense can be attached to suggestions like "Perhaps red objects really look green to Y", only if we make such translations as "Perhaps red objects look to Y just as green objects do": which amounts to the straightforward empirical possibility that Y cannot discriminate between red and green objects. But we must add that the sense in which red things look the same to X as they do to Y is different from the sense in which red things look alike to X, in that X can tell whether two things look alike to him just by looking; whereas it makes no sense to talk of anybody telling whether or not something looks alike to different persons in this way.

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Now there is some analogy between the way we use words like "pain" and the way we use words like "red". But if this fact is to be helpful we must not exaggerate it. It is obvious first of all that the use of "pain" is not related to *objects* as is the use of "red". We might indeed have had a use of "pain" in which we described such things as pins and red-hot pokers as "painful" or "pain-causing": and this use might have been such that to know the use of "pain" was simply to be able to discriminate correctly between painful and non-painful objects by feeling—*e.g.* by touching a poker or by trying a dentists' drill on one's teeth. This would have been quite like our normal use of "red"; unfortunately, it is not like our normal use of "pain".

We do, however, speak of "painful situations", "situations which cause pain". We might be tempted to say that to know the use of "pain" is to be able to discriminate correctly between painful and non-painful situations. But discriminating situations is not like discriminating objects. The same object can look red to both X and Y: but, in one obvious sense of "situation", the same situation cannot feel painful to both X and Y. Thus, X and Y agree in their use of "painful", not through applying the

word to the same situations, but through applying it to the same types of situations. We can say provisionally that for X to know the meaning of "pain" it is necessary and sufficient that: (1) He should feel, broadly speaking, the same in certain kinds of predicament. For example, his being burnt with a poker should normally feel to him, in general respects, the same as his being pricked with a pin or drilled by the dentist normally feels to him. (2) In other types of predicament, as when he is tickled with a feather, he should feel unlike this. (3) He should apply the word "painful" to situations of the first kinds, in virtue of their resemblance. (4) This classification of kinds of situations should, within reasonable limits, correspond with the normal use of "painful". We can then say that "I feel pain" means "I feel as I normally would feel in a painful situation"; and that to know what pain feels like is to be able to classify painful situations correctly, *i.e.* to know the use of "pain" as explained above.

But this is still wrong. For it is neither a necessary nor a sufficient condition of the correct use of "pain" that the speaker should agree with normal English speakers about the classification of situations as "painful" or "non-painful", even though he classifies them on the basis of his feeling in these various types of situation. Imagine a man who agrees that pins, pokers and drills feel much alike to him, and unlike feathers, but adds "Feathers cause me pain and pins, pokers and drills cause me tickles". We may be tempted to say that he has confused the words "pain" and "tickle". But suppose we then discover that on being tickled with a feather he cries out in apparent agony, while when he is burnt with a poker or drilled by the dentist his only reaction is to giggle helplessly? In such a case as this we might be inclined to say that his original statement was correct, although we might also be inclined to deny this and describe the situation by saying "He giggles when in pain and screams at tickles". But now suppose further that after he has been tickled the relevant parts of his body appear swollen and inflamed, and his condition improves only on the application of bandages and soothing ointments; while red-hot pokers have no such after-effects. Clearly this would incline us much more definitely to say that feathers really hurt him and red-hot pokers do not. We might still say this even though he himself had said that feathers tickled him and red-hot pokers caused him pain.

It is perhaps worth-while to contrast this with the kinds of circumstances in which we should say of a man that red objects normally look green to him while green objects look red to him. Clearly we should not say this, nor allow him to say it, unless he

showed some incapacity to discriminate normally between red and green objects—*e.g.* if red and green objects looked the same to him. There is indeed some openness of texture even here: what should we say if we discovered that his eyes were unusual in that their lenses always changed the colour of the light passing through them? Would this justify us in saying that red things look green to him, or would saying this be like saying "We all really see things upside down" simply because the image on the retina is inverted? But this is a small pinhole in our concept of "red" compared with the ragged rents in our use of "pain". We are sufficiently justified in saying that our use of "red" is governed by our ways of discriminating red objects, but that our use of "pain" is not similarly governed by our ways of discriminating the causes of pain.

This complicates the story, but without fundamentally altering its plot. For there are clear cases as well as doubtful ones, and if a person shows no abnormalities in his reactions to feathers and red-hot pokers or other relevant stimuli, but still says "Feathers cause me pain and red-hot pokers tickle me", then there is no puzzle: this is just misuse of the words "pain" and "tickle". Our normal use of words like "pain" depends on the fact that, with most people on most occasions, certain kinds of physiological causes tend to be associated with certain kinds of physiological effects and with certain types of behavioural reaction, and to have certain characteristic counter-causes (remedies). There may well be other relevant factors. It depends also on the facts that, for each of us, different circumstances which involve similar causes, effects, reactions, etc., tend to feel much alike to the victim; and that, on the whole, a range of types of circumstances C_1, C_2, C_3 . . . which feel alike to X will also feel alike to another person Y. That is, each of us can find a certain pattern of likenesses and differences among his feelings in different circumstances; the circumstances being specified with regard to effects, reactions, etc., as well as with regard to causes. Broadly speaking, each person's pattern of likenesses and differences tallies with that reported by others. These facts make it possible to lay down rules like: "To feel pain is to feel as one normally would feel in such circumstances as, *e.g.* being burnt with a red-hot poker and tending to scream", "To feel a tickle is to feel as one normally does feel in such circumstances as, *e.g.* being brushed with a feather and tending to giggle". Typical situations can, of course, be demonstrated rather than described.

This, of course, does not imply that such statements as "Red-

hot pokers cause pain " are linguistically necessary statements. To teach the use of " pain " we have indeed to make use of examples : but there is no example which *must* be used in this way. In the same way, we cannot teach a person the use of " red " without teaching him that some particular thing is red at that moment : but what red object(s) we choose for the purpose is a matter of indifference. There is no one thing, or class of things, which is the Standard Red.

Consequently, the task of establishing that X and Y attach the same meaning to " pain " is not barred by the insuperable difficulty of establishing that painful situations feel the same to X as they do to Y. For the question " Do X and Y feel alike ?" is the question " Would X and Y agree on the sorts of circumstances in which each would normally experience feelings like his present feelings ? " It does not and cannot mean " Are X's and Y's feelings alike in the sense in which X's feelings on one occasion may be like his own feelings on another occasion ? " ; for clearly it makes no sense to talk of anybody's comparing X's feelings with Y's in the way in which X can compare his own feelings with one another. But this does not mean that there is *no* sense in which the feelings of different persons are comparable.

II

The foregoing discussion may have served to ease the pressure of scepticism about other people's feelings ; but of course it does not suffice to remove it. The force of the well-known objections to the argument from analogy is lessened when we realise that this argument does not have to provide a proof that Jones has feelings like mine, in the sense in which my own feelings can be like or unlike one another : but the objections have not been answered. If what has been said is correct, the argument from analogy has the form " When I am in circumstances C, I feel as I would in circumstances D ; Jones is in circumstances C ; therefore, probably Jones feels as he would in circumstances D ". This taxes our credulity less than an argument of the form " When I am in circumstances C, I have a feeling F ; Jones is in C ; therefore, probably Jones has a feeling just like F ". But even so, it still makes no sense to talk of my checking the conclusion of such an argument. It still makes no sense to talk of any person other than Jones comparing how Jones feels in C with how Jones feels in D, in the way Jones himself can do this. And the difficulties that spring from this remain to be dealt with.

Before considering the argument from analogy, however, I

want to discuss the sort of difficulty that finds expression in the objection "How can statements about other people's feelings be meaningful unless they assert something publicly observable?" This difficulty does not necessarily arise merely from a die-hard allegiance to the Principle of Verification. It can be brought out fairly persuasively as follows: If the account in Part I is correct, it must make sense to say of any person X that he feels pain although he is not in fact in any of the outward circumstances with reference to which the use of "pain" can be explained; for it is not self-contradictory to say "X feels as though he were in circumstances C although he is not in fact in C". Likewise, it must make sense to say of X that he does not feel pain although his outward circumstances are in every respect such as to lead others to think that he does. Not only are these statements not self-contradictory, but we seem to have a definite use for them; for as we have seen, each of us can imagine what it would be to feel pain and not to manifest it in any way, or not to feel pain while in every way appearing to manifest it.

For ease of reference, I shall call the statements "Jones feels pain although nothing about him indicates that he does", and "Jones does not feel pain although everything about him indicates that he does", "S" and "T" respectively. We normally talk about pains in such a way that it makes sense to assert either S or T. But when we consider S and T in relation to speakers other than Jones, we may be tempted to ask how we *can* talk in such a way, how it *can* make sense to assert either S or T. For clearly, S and T are such that, even if either were true, no person other than Jones could ever have good reason to believe it true; so that the assertion of either by any person other than Jones, in any circumstances whatever, would be irrational. One does not need to be a fanatical Logical Positivist to agree that this is a puzzling situation.

Could *anybody* have a reason for saying that either S or T is true? One is to some extent inclined to deny this, because it is clearly silly to ask Jones his reasons for saying that he is or is not in pain. But if we do say this, we must also explain the way in which Jones' lack of reason for "I am in pain" is unlike Smith's lack of reason for either S or T, or anybody's lack or reason for such a statement as "there are totally unobservable pills in this bottle". The difference is that Jones' lack of reason does not make his statement "unreasonable" in the way that the other examples are unreasonable. It will be convenient, however, to employ a different use of "reason" for the purposes of this article, and I do not think this will seriously affect the argument.

I shall say that Jones does have a reason for " I am in pain ", and that this reason is his feeling of pain. This reason, of course, is unlike the sort of reason that Smith can have for " Jones is in pain " : it is foolish to ask Jones his reason for " I am in pain " because in saying " I am in pain " he has already given his reason, whereas Smith would be expected to give a distinct reason such as " Jones is groaning ". But the sense of " reason " in " Jones' reason " and " Smith's reason " is in part the same, in so far as in both uses " X does not have a reason for p " has the consequence that X is not justified in asserting p.

This convention enables us to say that the peculiarity of S and T is, that while there are circumstances which would constitute a reason for Jones' asserting either S or T, there are no circumstances which would constitute a reason for the assertion of either by anybody else. So, *can* S and T be significant ? There is something naïve about the question. How could it be answered with either a simple " yes " or a simple " no " ? For the whole point about S and T is : (1) They are not like statements which anybody might have reason to believe, such as " This bottle contains pills although the label doesn't say so ". (2) Nor yet are they like statements which nobody could ever have reason to believe, such as " This bottle contains wholly unobservable pills ". The question, in fact, is one which can only be answered by deciding how the word " meaningful " shall be used. We might put the matter this way : there are rules for the proper use of S and T by Jones, but no rules in accordance with which either could properly be used by anybody else. So how can we give a simple yes-or-no answer to the question " Are there rules for the proper use of S and T, or aren't there ? "

Of course, this oversimplifies the problem. A decision about the use of " meaningful " is not *just* a decision about the use of a single word, or even of a group of related words. Somebody may say : " Surely, the question is whether or not there are, or could be, any *facts* describable by S or T ? " But this is mistaken. For to say that an expression is significant is not to say that it is ever in fact true, or that, except in the sense of " logically could ", it ever could be true. Whatever we decide about the use of " meaningful " nobody will be committed to holding that it is physiologically or psychologically possible for either S or T to be true. If it were possible, there really would be a problem about other minds.

On the other hand, it certainly is the case that if we say that S and T are meaningless, it follows that neither can be true. But

this, obviously, is not because the statement "S is meaningless" is a statement about non-linguistic facts. On the contrary, to say there are no facts such as S describes is to say that S is significant, but false; to say that S is meaningless is to say that, whatever facts there may or may not be, S cannot properly be used to describe anything at all. Now in some cases the meaninglessness of an expression, in this sense, follows from logical or linguistic rules, other than the rules for the use of the word "meaningful" itself; thus, that "this is a round square" is meaningless follows from the use of the words "round" and "square". But the position of "S" and "T" is not analogous to this. They are not self-contradictory expressions, and the difficulty is not simply one of understanding their internal logical structure.

The source of the trouble is perhaps as follows. To decide to use the word "meaningful" in such a way as to entail that an expression E is meaningless, is in effect to blacklist E; to decide that, even though E may not break the syntactical rules of language, it shall never be proper for anybody to describe any state of affairs by means of E. This decision may be called "a decision that language shall not include E". Milder decisions of the same sort are possible: we might decide that E may be used, but shall be regarded as expressing a picture-preference and not as a true-or-false description of any state of affairs. But the differences between these decisions are unimportant for our present purpose. Now, a decision of this type will not affect the furniture of the universe: but it will affect our inventories. Consequently, such a decision, although not a decision about the contents of the non-linguistic world, may still be reasonable or unreasonable in view of the nature of those contents. We *could* make our language too narrow, and render ourselves unable to describe some imaginable state of affairs; that is, we might deny ourselves the right to use E, although circumstances are imaginable in which, in the absence of this restriction, somebody would have reason to assert E. Of course, it also follows that if a language is too narrow, we cannot say so in the same language: we cannot list the actual, possible or imaginable states of affairs which our language does not permit us to describe.

On the other hand, it would also be possible to use "meaningful" in such a way as to make our language too broad. That is, we might permit language to include statements which are useless in that nobody could ever have reason to assert them; and it is this possibility which gives point to philosophical "criteria of meaning" such as the Principle of Verification. We can make

our linguistic dress so tight that it hampers our movements; or we can make it so voluminous that it flaps about our ankles. There is nothing to stop us doing either of these things; but they are both silly things to do.

In the light of the foregoing, we can consider some of the possible ways of using "meaningful" in relation to S and T, and the sorts of language-structures that result.

(A) We might decide to use "meaningful" in such a way that an expression E is meaningless, unless it is such that *anybody* might have reason to assert E; unless there are rules in accordance with which *anybody* can use E. We shall then have to say that S and T are meaningless, for all speakers including Jones. We shall say the same of the corresponding statements about each of us: *e.g.* "Mellor feels pain though nothing about him indicates that he does", "Immanuel Kant is in pain although nothing about him indicates that he is", etc.. But clearly such a language will be too narrow, in the sense explained above; for each of us will be able to imagine a situation which his language does not permit him to describe.

(B) We might, and are often tempted to, use "meaningful" in such a way that E is meaningful for X, only if X could have reason to assert it; and meaningless for Y, if E is such that Y could never have reason to assert it. We shall then say that S and T are meaningful for Jones, but not for any other speaker; that "Mellor is in pain although nothing about him shows it" is meaningful for me, Mellor, but not for any other speaker; and so on. This avoids the difficulties of (A); but at a price. We now have a series of what may be called egocentric languages, each private to one speaker; and there can be no intersubjective language in which propositions about feelings can occur. For if S and T can be significant only to Jones, there can be propositions corresponding to them only in the language used by Jones, and not in the language used by anybody else.

(C) But there is no reason to talk in either of these ways. We can quite easily use "meaningful" in such a way as to say that E is meaningful for any speaker, provided *somebody* could have a reason for E. We shall then have an intersubjective language, in which the significance of expressions does not depend upon the identity of the speaker, and in which the expression ". . . is in pain" can be used in the way in which it does, in fact, appear to be used in everyday language. Nor will such a language be too broad, in the sense discussed above; it will not let in metaphysics by the back door, if metaphysical statements are regarded as statements which nobody could ever have reason

to assert, or for whose proper use by anybody there are no rules at all. For there *are* rules in accordance with which Jones can use S and T, and there are rules in accordance with which each of us can use corresponding expressions of himself.

There may still, however, be a feeling that the question cannot be settled merely by adopting a convention about the use of "meaningful"; people may be tempted to ask if we *can* make use of an intersubjective language of the sort described in (C). I shall notice two possible sources of this difficulty.

First, somebody may say that the specification for an intersubjective language laid down in (C) begs the question with regard to the reality of other people's feelings. For the reason given in (C) for saying that S and T are significant is that under certain conditions Jones would have reason to assert S or T. But if this could not be true unless Jones did in fact sometimes have feelings of some kind, the argument would be circular. However, it is plain that the statement "under certain conditions Jones would have reason to assert S or T" does not imply that such conditions are ever in fact satisfied.

Secondly, somebody may ask "How can anybody learn the meaning of an expression if there are no rules in accordance with which he himself can use it? How can anybody understand a statement which is such that he himself could never have a reason for believing it true?" The answer to this is simply "Why not?" For the question does not point out, as perhaps it appears to do, a psychological impossibility; it is a linguistic question, of the form "Ought we to call this 'learning the meaning' or 'understanding'?" Children do not find it psychologically difficult to learn the meaning of expressions like "totally invisible fairies"; but philosophically inclined adults may experience a reluctance to say what the child does is *really* learning the meaning of, or coming to understand, an expression; they may prefer to call it "learning to express a picture-fondness". A similar reluctance may be experienced in connexion with S and T. Yet, since S and T are different from statements about absolutely invisible fairies in the ways already explained, this reluctance should not be insuperable. Each of us learns the rules for using expressions corresponding to S and T of himself; so each of us learns the rules in accordance with which Jones can use S and T; and this is in some ways like learning the use of a straightforward, non-puzzling, expression, and in other ways unlike it. Once more, we have to decide what our usage is to be.

We may sum up this part of the discussion by saying that the puzzle "How can statements about other people's feelings be

significant ? " arises from a difficulty in deciding how words like " meaningful ", " understanding ", " learning the meaning of ", should be used. To decide this is to decide the scope of our language ; and such a decision may be for good or evil, but it cannot be either correct or mistaken. I, Mellor, may conceivably be mistaken in supposing that any statement about the feelings of anyone other than myself is ever in fact true ; but there is nothing to stop me using a language which contains such statements.

III

The preceding discussion has dealt with problems about the meaning of statements like " Jones is in pain " ; we have yet to consider questions about their truth. That is, we have still to face the problem : Is the argument from analogy a good inference ? Even if our beliefs about other people's feelings are significant, can they be justified by probable reasoning ?

One way of answering such a question would be to show that the methods of inference we use to establish our beliefs about other people's feelings are simply special cases of ordinary inductive inference. There is certainly a close resemblance between symptom-feeling inferences and ordinary inductive inferences. Nevertheless, I shall argue, the resemblance is not perfect, and a man who is prepared to accept the conclusions of inductive arguments can consistently, though not reasonably, refuse to accept the conclusions of analogical inferences about other people's feelings.

Mr. Stuart Hampshire, in his article " The Analogy of Feeling " (MIND, Jan. 1952) seems to be trying to show that symptom-feeling inferences are normal inductions. He opens his article with the claim that " statements about other people's feelings and sensations " can be " justified . . . as being inferences from the observed to the unobserved of a familiar and accepted form ". He points out later in the article that each of us is in a position to check the inferences made by others to his own feelings ; and he lays down the important principle that " all that is required for testing the validity of any method of factual inference is that each one of us should *sometimes* be in a position to confront the conclusions of the doubtful method of inference with what is known by him to be true independently of the method of inference in question " (my italics). Now, Hampshire certainly succeeds in showing that this applies to the inferences we make about one another's feelings ; and it is beyond question that it

also applies to ordinary inductive inferences. But this is not enough to prove that symptom-feeling inference is a species of ordinary induction. For the standard objection to symptom-feeling inference is not that each of us is never in a position to check the conclusion of *any* such inference, but that it makes no sense to talk of his independently checking the particular inferences that he himself makes about others. I think Hampshire would admit this point: he says "naturally, on those occasions when [each of us] needs to use any particular method of inference, he cannot be independently checking the inference on the same occasion". But this is not the only trouble with Smith's inference to Jones' pain. When Smith infers from the smoke above the trees that a train is about to round the curve, he cannot be independently checking the inference at the same time; but after having made the inference he can still race to the level-crossing to check it. This is just the kind of thing it makes no sense to talk of doing in connexion with his inference about Jones' pain.

Now, this is really a trivial objection: the resemblance Hampshire has pointed out between symptom-feeling inferences and ordinary inductions is more important than the differences. A bird in hand is worth two in principle, and the fact that we have actually tested a certain method of inference by checking some examples of its use is a better reason for relying on it than the fact that we might conceivably have tested any example. Nobody would justify his inference from the clouds to the impending rain by saying that, if rain does occur, it will make sense to talk of his observing it; although anybody might justify such an inference by saying that on some other occasions when such clouds have been about he has in fact observed rain to follow. We might say that Hampshire has laid down a criterion for "reliable method of factual inference" which ought to be enough to satisfy any reasonable man. But what is enough for reasonable men is not always enough for philosophers. A sufficiently determined sceptic could, without contradiction, reject Hampshire's assimilation of symptom-feeling inference to ordinary induction, and refuse to accept the conclusions of the former while being quite happy about those of the latter, on the ground that ordinary inductions satisfy a condition which is not satisfied by symptom-feeling inferences. In what follows I shall try to bring independent arguments against such a position.

It is worthwhile to compare the position in question with that of a man who refuses to accept the conclusions of ordinary inductive inference. Suppose somebody asks: (*a*) "What makes

the inference from clouds to rain probable ? ", (*b*) " What makes the inference from Jones' groans to his feeling of pain probable ? " In both cases we can answer by pointing out that other inferences of a similar character have in fact been successful, or would have been successful had they been made. Thus, we can say : (*a*) " On other occasions when such clouds have been about rain has been observed to follow " ; (*b*) " On other occasions when I myself have exhibited similar symptoms others have, or might have, correctly inferred that I was in pain ". Answer (*a*) might have been paraphrased by " It's probable because it's an induction from past experience " ; answer (*b*) might have been paraphrased by " It's probable because it's an argument from analogy with my own experience ". Now in both cases there is a temptation to raise the further questions : (a_1) " What makes inductions probable ?", (b_1) " What makes analogical inferences probable ? " These questions run into similar difficulties. If we say " Inductive conclusions are probable because inductions have often been successful in the past ", we are faced with the question " But how do you know that inductions will be successful in the future—except by induction ? " Likewise, if we say " Analogical conclusions are probable because the analogical arguments made by others about myself are often successful ", we get the question " But how do you know that the arguments you yourself make about others are also successful—except by analogy ? " Just as it is not possible to show independently of induction that the future will be like the past, so it is not possible to show independently of analogy that others are like myself.

The way to deal with this situation in connexion with the Problem of Induction is by now familiar. The justification of induction cannot depend on showing that inductions will continue to be successful, since this would be circular. Instead we can maintain that the statement " Inductions are probable (reasonable) " is analytic [1] and that consequently there is no problem of justifying induction. In the same way, the justification of the argument from analogy cannot depend on showing that this argument does in fact yield true conclusions about others than the user of the argument ; for this can only be shown, to the satisfaction of the argument-user, by means of analogy. Correspondingly, we must maintain that the statement " the argument from analogy yields probable conclusions (conclusions it is reasonable to accept) " is analytic : for we should refuse to call a person " reasonable " unless he did, in appropriate circumstances, accept the conclusion of an analogical inference.

[1] Cf. Strawson, " *Introduction to Logical Theory* ", pp. 256 ff.

There is still a difference between the statements "Inductions will continue to be successful" and "My arguments from analogy about other people are often successful". This difference, however, is just what shows the Problem of Induction and the Problem of Other Minds to be parallel, rather than identical, problems. Although it is true that the first of these statements cannot be established independently of induction, it is also true that, in the case of any particular inductive prediction which relates to a determinate future time, the future will eventually show whether it has been successful or not. Any particular inductive argument can be independently checked, provided its conclusion is such as to be checkable within a finite time; although obviously the whole class of inductive inferences cannot be so checked. This, of course, is what makes it possible to have inductive support for the statement "Inductions will continue to be successful". But it is not possible for Smith to check independently the conclusion of any particular analogical inference by which he infers the feelings of another. If it were, it would be possible for Smith to have ordinary inductive support for the statement that the analogical inferences he makes about others are successful. Since this is not possible, the only support he can have is itself analogical; and in this way the class of "inferences about the future" is in a similar position to the class of "inferences made by X about the feelings of people other than X". There can be no general proof that inferences of either class are successful, which is independent of the method of inference concerned.

We can use this to point the moral. The sceptic about induction knows that a certain number of inductive inferences have been successful; but he still wants to say "But future inductions may be different". In saying this he shows that he refuses to accept inductive standards of proof; he hesitates at the step from past to future inductions, just because it is non-deductive. In a parallel way, the sceptic about other minds knows that a certain number of symptom-feeling inferences have in fact been successful; namely, those made by others about himself. But he still wants to say "Inferences made by me about other people may be different". In saying this, he shows that he refuses to accept analogical standards of proof; he hesitates at the step from inferences about himself to inferences made by himself about others, just because it is not an ordinary-inductive step—it cannot be made by one who is unwilling to argue by analogy with his own experience. The sceptic about other minds is insisting that all proof shall be ordinary-inductive, in much the way that the sceptic about induction is insisting that all proof

shall be deductive. In both cases, there is nothing we can do to help except to say that such an insistence is, by definition, unreasonable. The man who says "But I don't want to know what people call 'reasonable', I want to know whether the inferences that worry me have true conclusions" cannot be comforted. Nobody can prove anything to anybody who refuses to accept the appropriate standards of proof.

V.—WHY WE CANNOT WITNESS OR OBSERVE WHAT GOES ON 'IN OUR HEADS'

By H. Hudson

Why is it logically absurd to say that those activities and performances which are said to go on, or to be carried out 'in our heads' could be the objects of sensory observation? My aim is to try to solve this problem. The traditional doctrine about our minds and their relation to our bodies appeared to provide an answer to it; for if our minds do not exist in space then to suppose that sensory observation of their workings is possible, would obviously be absurd. But to a large extent the pathology of the traditional doctrine has already been elucidated, and I have no intention of attempting an additional clinical or post-mortem examination of it here. However, if the traditional doctrine is rejected we are left with the problem mentioned.

For a long time it has been customary to say that 'mental' activities are 'logically unobservable', indeed this has been regarded as an indispensable part of what is meant by characterising any activity as 'mental'. Such a view is incorrect. We may 'think aloud'; 'ponder or deliberate aloud'; 'indulge in reverie aloud'; 'decide aloud'; 'remember aloud'; 'picture something by using a brush and paints or a pencil and paper', or 'try to work out' what something would look like by building a model, or by using a brush and paints; 'calculate aloud or by using pencil and paper'; and so on. We may do all such things without having to conduct another performance simultaneously, privily and in silence, 'behind' as it were, the overt front. All such activities can also be carried out 'in our heads'; but to say that any kind of activity can be carried out 'in our heads' would be quite pointless unless it could also be carried out overtly. We may conclude then, that every *kind* of activity which we say can be carried out 'in our heads' can also be carried out overtly; and as Ryle has pointed out, these overt activities are not 'counterparts' of 'mental' activities, they are 'mental' activities.

Philosophers in particular have been prone to think that what goes on 'in our heads' consists of task activities such as 'planning'; 'calculating'; 'pondering'; 'deliberating'; and so on. They have tended to ignore or overlook the frivolity, fool-

ishness, and at least apparent pointlessness, of a good deal of what goes on 'there'. Words, phrases, and remarks, keep coming 'into our heads' with obsessional frequency; jingles, rhymes, and tunes, run 'in our heads' for no obvious reason; while 'reverie' and 'day-dreaming' for instance, can hardly be regarded as the names of task activities. Nevertheless, most of these things are either the results or typical components of task activities; or were once components of task activities; or are typical associates of them. The tunes, verses, rhymes, and jingles were once memorised, worked out, or 'hit upon'; many of the remarks, observations, and comments were once involved in solving a problem and could be involved again; others could be to the point in some task activity but are pointless if no such activity is being undertaken; and the exclamations which express annoyance, surprise, disgust, triumph and so on, are familiar accompaniments to carrying out tasks. Such things seem to pop up in a haphazard, aimless way, almost as if they had lost their place or strayed from their vocation, and were kept going by some mysterious inertia of their own.

Whereas young children will drift naturally into reverie and day-dreaming, for instance, they have to find out how to work or think out problems 'in their heads'; and how to remember remarks, instructions, and tunes, by going through them 'in their heads'. To suggest that the young child of three, for instance, never has anything going on 'in his head' or 'running through it' would, I think, be quite mistaken. But I do think that he has to learn how to keep something 'in his head', and not merely learn how to do things 'in his head' but how to do them whenever he wishes or whenever circumstances require it—for certainly there is a great difference between 'doing things in one's head' and merely having them 'running through it' or 'going on in it'. Moreover, it is mastery of doing things 'in our heads' that increases fluency in the production of the more or less aimless nonsense and non-task activities mentioned. Although the application of the expression 'in the head' should not be confined to task activities, nevertheless it does seem to have a predominantly task significance. Accordingly, I shall be concerned almost entirely with task activities which are said to be carried out 'in our heads', and on this account to be logically unobservable; although at the end of this paper I shall attempt to apply what I have said to the other activities as well.

Perhaps someone will have the impression from what has been said that only those activities which are said to go on, or to be carried out, 'in our heads' are logically unobservable. Such an

impression would be quite mistaken. Our perceptions are logically unobservable, but to say that one smells, hears, or sees, ' in one's head ' ; or that smelling, hearing, or seeing, go on ' there ' is patently absurd. As Ryle has pointed out, perception verbs are achievement verbs and do not stand for any special act or activity or for any way of being occupied. Again, I may ' see ' as if with my eyes or ' see in my mind's eye ' ; ' hear as if with my ears ' ; ' feel as if I were touching velvet with my fingers ' ; ' feel as if I were moving my lip and tongue ' ; and so on. But if it is nonsense to say that literally seeing with my eyes, hearing with my ears, and so on, take place ' in my head ', then it does not make any better sense to say that not literally doing these things takes place ' there '.

Expressions such as : ' seeing as if with my eyes ' or ' seeing in my mind's eye ' ; ' hearing as if with my ears ' ; and such like ; or the more general ' having an image ' ; are ' achievement ' or ' got it ' expressions. Images may be ' evoked ' or ' called-up ' ; ' conjured up ' or ' formed '. We may try or try hard to evoke or to form images and may succeed or fail in doing so. Images may also come unbidden or unsought and they may also be unwanted. We may not require them for any task with which we are occupied, or for various reasons we may want to get rid of them. But we may get things without trying to get them, and find things without trying to find them. Similarly we may get images without trying to get them, or without trying to evoke, conjure them up, or form them.

As soon as we say that we ' see something in our mind's eye ' we could also say that we ' have seen it ', and this applies whether the ' seeing something in our mind's eye ' is evoked or formed, or unsought or unwanted. Nor can we say that we ' see something in our mind's eye ', ' attentively ' ; ' carefully ' or ' pertinaciously ' ; although we could say that a man may ' keep his mind on the job ' when he is trying to ' call up ' or ' form ' an image ; that he may be very tenacious in trying to ' call up ' or ' form ' one ; or that in certain circumstances when he is trying to picture or visualise something, he may be careful.

Learning various *kinds* of overt performances and the reduction of these to routines is an indispensable preliminary to the performance of various affiliated kinds of activities in our heads. To acquire the drills and routines of ordinary arithmetic by working out sums on paper or on a blackboard ; by counting aloud ; by learning tables by heart by chanting them aloud ; and so on ; is an indispensable condition for performing feats of

multiplication, addition, etc., in our heads ; and the greater our mastery of the routines, the easier it is to carry out these operations in such a way. To say that little Willie can do multiplication in his head but cannot and never has been able to do it on paper or in any other overt way, would not merely be odd, it would be absurd.

There are a few children who not only master the overt routines with surprising speed, but are subsequently able to carry out lengthy calculations in their heads with amazing fluency, speed, and accuracy. But on the whole, young children find the performance of simple calculations in their heads laborious, tedious, and uncertain. A capacity to do these things in one's head is certainly regarded as some measure of one's mastery of the art of reckoning, in much the way that a capacity to read silently is taken as a measure of the child's mastery of the art of reading.

The flow of verbal, kinaesthetic, auditory, and visual, imagery which makes up such a lot of our thinking, when we refrain from thinking aloud, would not be possible unless we had acquired some mastery of the art of using words in talking and writing; or unless we had learned certain perceptual lessons about the use of our eyes and ears, such as the fixation of objects, estimation of their distance in terms of reaching and walking, etc. ; and so acquired various observational skills of which the characteristic perceptual successes or achievements are the outcome.

That the overt performances come first can be seen from a brief consideration of those activities which we carry out in our heads. When we perform these activities we make use of different kinds of imagery ; but imagery of what ? Of numbers seen on white paper, for instance ; of making movements involved in writing or talking ; of hearing the sound of one's own voice ; and so on. To have this imagery involves that one knows what numbers look like ; that one knows what it is to write and combine them ; that one knows what it is to combine words sensibly and correctly—that one knows what it is to talk ; and so on ; that not only does one know how to write and say words and numbers, but that one knows how to *use* them. In short, to have this imagery and to be able to make use of it, presupposes a prior acquaintance with the kind of things of which one has images, and with carrying out the tasks in question. To have images is an achievement of which the occurrence of various perceptual achievements is a condition. To be able to make use of these images involves that one must already know how to carry out the tasks overtly, because it is the component sounds, sights, and movements, involved in carrying out these tasks overtly of

which we have the imagery. So, knowing how to do things in one's head—and a very young child does not know how to do this but has to find out—logically presupposes knowing how to do them overtly.

In so far as we succeed in doing things in our head we also succeed in inhibiting the normal movements of our lips, tongues, hands, fingers, and so on, made in writing, humming, dancing, speaking, etc. Indeed when we have mastered the art of doing things in our heads we can dispense with all such movements. But as nearly everyone knows, when our mastery is incomplete, or when confronted with a difficult problem, we do make movements with our lips, tongues, and hands; mutter to ourselves or under our breath; and may occasionally catch ourselves doing these things and make an effort to suppress or inhibit them.

To learn or discover how to inhibit certain movements is to learn or discover how to produce other movements. At times we are a little apt to forget that movements are inhibited only by producing others. Someone may suggest that some so-called 'inhibitory movements' are just 'checkings of movements', they are not really movements at all. Certainly, if I am on the point of thrusting my hand into my pocket to see if I have my car keys but inhibit the movement because I remember that they are there, it does seem that all that has happened so far as movement is concerned is the checking of a movement. On the other hand, the checking of a movement which I am 'muscularly set' to perform and on the point of performing, can be accomplished only by producing a certain contraction of the relevant muscles; and to produce this muscular contraction is to produce a movement of a different kind from the one inhibited. The checking or inhibition of the movement in question is the outcome of effort and of making another sort of movement successfully. This way of inhibiting movements by producing muscular contractions in the limbs or organ about to be moved, has to be learnt. Young children, for instance, are poor performers, and their lack of proficiency gets them into a good deal of trouble with their elders. To say that inhibitory movements must be different from the movements they inhibit would be tautological although not without importance in this context. Accordingly, to say that the inhibitory movements are copies or replicas of the movements they inhibit would be incorrect; nor can they be regarded as counterparts of these latter.

'Inhibiting' is not a first order achievement verb, but a second order one; at least it is in the contexts in which I am

using it. Inhibition cannot occur until there is some movement or activity to inhibit; nor, with the exception of certain reflex movements, is it logically possible for a person to find out how to inhibit an activity until he has already found out how to perform the activity which he has to inhibit—and has some mastery over it, for otherwise he would be unable to keep his mind on the job. Success in performing the activity to be inhibited is, then, a logical condition of inhibiting the activity in question successfully. Inhibiting is the successful culmination of certain activities or the successful completion of some task. It does not designate some special activity or act, or way of being occupied. Nevertheless we often use the word in order to characterise actions and efforts in terms of the kind of achievement to which they have led. This practice is very common; for instance, we often use words such as 'planning'; 'deciding'; and 'deducing'; in the same way.

To say that in order to work out sums or to conduct monologues in my head I must acquire the habit of inhibiting the use of my lips, tongue, hands, and the rest, does not mean that no other movements are made, or that I make no other movements, for doing these things, and that I therefore pluck my answers and solutions out of the occult air so to speak, or from some backstage occult performance. It means, among other things, that I make different movements which inhibit the former ones. If A is a given movement and A′ its inhibitory movement, then to say that S could do A and A′ simultaneously would be self-contradictory. (Try spelling 'apple' in your head and aloud simultaneously.)

We would not be entitled to conclude that these inhibitory movements are logically unobservable. Indeed *qua* movements they will be observable, at least in principle. In fact these movements happen to be minute: small muscular contractions and movements in the tongue, larynx, eyes, and fingers, for instance; which could be and have been recorded by instruments. Now doing something in one's head involves making these inhibitory movements, although making them *does not constitute* doing something in one's head. Why is it then, that although we could still observe the movements involved in carrying out a task when a person does it in his head, we cannot be said to observe or witness his carrying out of it? It is quite different, for instance, when we watch him multiplying 475 by 5 on paper, or listen to him reasoning something out aloud. In such situations as these we can tell what he is doing and the way he is doing it,

by watching his movements and keeping an eye on his results, or by listening to the sound of his voice and taking note of what he says.

We are often able to tell what is going on in another person's head, and are often able to do so in virtue of movements whic' express or exhibit his attitudes to what is going on 'in his head', or to the sort of thing he is doing. When he is thinking; planning; or pondering; for instance, his face may take on a thoughtful expression or a puzzled look. But taking on these expressions, and others expressing surprise, disappointment, frustration, relief and satisfaction, involve making facial movements which, as such, are not involve in the business of finding solutions and clearing up worries. The are movements of quite a different kind. Of course, we do n rely solely upon movements of such kinds to tell us what is going on in another person's head; various contextual considerations, and what we already know about the person in question, are frequently used. But although we may know, or be able to tell, what is going on in another person's head, it remains absurd to say that we could do so by observing what he is doing when he is working out something in his head.

The full dress overt movements involved in carrying out the kinds of tasks with which we are concerned, such as calculating; planning; deciding, and so on, have the important feature that some of them are ways of giving results as well as of getting them. The results are given or perhaps presented, in the sense that they are there or available, if anyone wants to look for them; but not in the sense that they are explicitly or deliberately provided for some observer. For it is clear enough that when what we do involves giving or presenting results, we are often quite indifferent whether or not any other observer is present to witness them. Thus in multiplying 597 by 5, the actual working out of the answer, by using words, or by using pencil and paper, involves at the same time giving the results or parts of the answer; and until these are given, and unless we know the rules of arithmetic, we are unable to tell what the person concerned is doing. Again, arguing aloud and in a full dress overt manner in order to show that some assertion is correct, involves making movements which produce sounds in a certain arrangement and sequence; but in doing this, one is at the same time involved in giving, presenting, or delivering, the argument for the assertion in question.

An important, if not indispensable, point about doing things in our heads by inhibiting the full dress overt movements, is

that by so doing we are able to inhibit giving the results of what we do. But the reason for this is not that we shun publicity or are disinclined to do what could, at any time, be subject by inspection to the judgement of another person—although at times these could be reasons. The main reason is the very humdrum one that it is easier to do one thing at a time; and that results may be got without being given. Often, it is this latter point which strikes or dawns on the young child who is acquiring the knack of doing things in his head; for example, he may say: "I can do it now so that you can't hear me". Moreover, since results cannot be given until they have been obtained, then getting them is a first priority. To get the solution is to be able to give it, or at least to be in a position to give it; but to be able to give what the correct solution would be does not involve that one is able to get it, or to work it out. One may know what the correct solution would be, but be quite unable to work it out, or show how it is obtained. Besides, there would be no sense in saying that results are given unless there was some genuine possibility that they could be withheld, or that one could abstain from giving them. Giving results and getting them are therefore logically independent even although there are movements which are at the same time ways of getting results and ways of giving them.

By inhibiting the full dress overt movements (and considerations of ease and convenience lead us to do this), we are also simultaneously inhibiting the movements involved in giving the results of what we are doing; for many of the movements involved in giving the results are also involved in getting them. This is why our efforts to do things 'just to ourselves'; 'saying it just to oneself'; 'thinking it just to oneself'; 'working it out just to oneself'; can be successful. The expression 'just to oneself' could often, although not always, be profitably substituted for 'in one's head'; and sometimes it is, in everyday talk.

Since a given movement and its inhibitory movement cannot be produced simultaneously, movements cannot be inhibited until they are actually under way or being produced; *i.e.* the beginning of the movements to be inhibited must pre-date the occurrence of the inhibiting movements. (The adopting of certain 'muscular sets' which are preparatory to making a given movement may be regarded as being involved in its production.) So far as talking is concerned, for instance, we must have initiated the talking movements before we inhibit them. Hence the aptness of expressions such as 'sub-vocal speech'; 'talking to

oneself'; 'silent monologues'; and so on; and the elusive similarity and difference between doing something in our heads and saying it aloud. Our ability to make the inhibiting movements is not only a condition of our ability to do things in our heads, it is also *a* condition of the occurrence of the 'verbal' and other kinds of imaging and imagining that go on when we do something in our heads. We tend to think of inhibiting in terms of 'response and check'; and certainly our first endeavours to work out things in our heads are slow and laborious. But after a time the responses and checking movements become integrated into single complex movements which remain inhibitory of fully overt performances. They become increasingly fast and facile, and become habitual in certain contexts and for certain purposes.

What is the relation between these inhibitory movements and activities and other kinds of imagery? This question may well be raised with respect to visual imaging and imagining; for there is no doubt that a good many people depend much more on visual imaging and imagining when they work out problems in their heads, than they do upon verbal imaging. When the meaning of a word is required we may give its uses; or we may point to some individual, or to a likeness of some individual such as a portrait, picture, sketch, or model; *i.e.* we may define the word ostensively. Again, instead of giving us a verbal description of some individual, incident, or scene, a person may draw a sketch, paint a picture, or make a model. If he does this we could perhaps say that instead of giving a verbal characterisation of the thing in question, he has given an ostensive one.

Now when a person is required to solve a problem but inhibits the full dress overt movements and activities which would enable him to solve it, then he may react in a verbal manner; *i.e.* he may try to solve the problem by means of verbal imaging. On the other hand he may react in what I shall call an 'ostensive' manner. For example, in order to solve a problem he may depend upon a series of visual imagings and imaginings of individuals, places, and so on. These may be symbolic, in the sense that they stand for a kind or class of thing; or they may be symbolic, in the sense that they stand for some individual or incident; depending upon the kind of problem with which the person is concerned. One thing is quite clear; if the full dress overt movements and performances are inhibited, and the person still intends to work out the solution of his problem, then he must engage in some substitute performance and this must take the form of either verbal imaging and imagining, or of ostensive

imaging and imagining, or it may be partly both. So the habit of inhibiting full dress overt movements and activities is a logical condition of doing things in our heads when we work largely or partly by visual imaging and imagining or by any other kind of imaging and imagining.

In many, although by no means all, problem contexts these sequences of ostensive visual imagings and imaginings are substitutes for verbal performances; and in many instances they will have the substitute movements for talking as an accompaniment. In passing, it is worth mentioning that when engaged in visual imaging and imagining, movements of the eye muscles involved in focusing and fixating objects usually occur. This would explain why we often feel ourselves to be watching, looking at, or even staring at what we get in our visual imagings and imaginings; or that we are keeping what we get under some sort of observation.

If we could not tell what sort of results or achievements are following from making various movements or performing various operations with words, numbers, and so on, then we would be at a loss to say just what the person was doing. Even if we allowed the person to state: '$547 \times 5 = ?$' for instance, we would be unable to tell from his succeeding activity whether he was or was not doing arithmetic, swearing to himself, or merely letting the muscles of his larynx vibrate. If his way of getting results does not involve giving, presenting, or delivering any, then an observer has nothing of which he can take hold. But it may well be said that these last remarks depend upon making use of the conclusion which I have set myself to establish; namely that what goes on, or is done in our heads, is logically unobservable. I may be asked: "How can you be sure that these complex inhibiting movements of which you speak, are always 'getting movements' and not 'giving ones'; or are not themselves 'getting—giving movements' of another kind?" In order to answer this question we had better ask as a preliminary: 'What is dispensable, and what indispensable about the full dress overt activities?'

The characteristic feature of calculating, pondering, deliberating, picturing,[1] memorising, and so on; the kind of activities with which we are concerned here, whether practised in overt form or not, is the getting or attempted getting of results. They are ways of setting about or carrying out tasks. Whether

[1] I am using 'picturing' in the sense of working out, or trying to work out, what something looks like, or would look like.

results are given in the execution of these activities or as the termination of them is contingent or incidental. Getting an answer is not the same thing as giving it, although it does involve a capacity to give it if required ; and may incidentally or contingently be at the same time a way of giving it. Accordingly the ' giving ' or ' presentational ' character of the full dress overt forms of those activities with which we are concerned is a dispensable characteristic, whereas their ' getting ' character is not. Since the inhibitory movements are involved in carrying out the same kinds of tasks as the full dress overt ones ; and since these former movements must be different from the latter ; we will conclude that what is dispensable about the way of carrying out these tasks is what is eliminated by inhibition ; for inhibition does involve the elimination of certain responses by substituting others.

We have not solved the problem yet although I think we are within sight of what may be a solution. In order to reach a conclusion I shall have to make use of some points which Ryle has made about ' achievement verbs ', with which most readers are doubtless familiar. An achievement is something over and above the activities which bring it about. While watching a race we may see a man touch the tape ahead of the other competitors, but while his doing this is an achievement, it does not mean that he has won the race. What he has done while running must conform to various rules ; and although seeing him come first ' presents ' us with the information necessary for issuing a verdict, we are not in a position to issue a verdict unless we know the rules. One does not say : ' I saw him win ', at least not in the same sense in which one says : ' I saw him reach forward and touch the tape before any other competitor did ' ; but if the rules have been observed, then to say this latter, means that ' I saw him win ' ; and as soon as he *does reach* forward and touch the tape before the others, then he *has won*.

To try to track down his ' act ' of winning as if it were some act over and above and distinct from that of touching the tape first in accordance with the rules of racing, is to try to track down something that is not there. It is not the sort of thing to be tracked down by observation or by any other form of detection. If anything did have to be tracked down, it would not be some mysterious and elusive act which disappears into the past as we try to come to grips with it, but some rule or rules of racing.

We are particularly prone to characterise activities and tasks in terms of the kind of results or achievements to which they give rise. The kind of activities with which we are concerned

here are no exception to this. Expressions such as 'planning', 'thinking out', 'solving', 'deciding', 'doing mental arithmetic', have both a task and an achievement significance. Construed as achievement verbs or expressions, they designate either the final or terminating result we get, or the results we get *en route* to the terminus, or both. But the acts or processes for which we may mistakenly think they stand are systematically elusive. There are no such acts or processes and it is logically impossible to observe any. This is by no means the whole story however, since the terms and expressions in question have a task as well as an achievement significance.

When a person does something in his head we may watch or record the movements involved in doing it, but by so doing we cannot obtain any information which would enable us to issue any verdict about what he has succeeded in doing or has failed to do. Observation of the movements in question cannot give us the necessary information. In order to understand this, we must appeal not merely to the fact that the person has to make inhibitory movements if he is to carry out a task in his head; and to what is eliminated and dispensed with as a result of inhibition; we must appeal to another point which lies, so to speak, behind this latter point: *i.e.* to what he 'gets' by making these inhibitory movements. (Compare: What he 'gets' by making certain eye and head movements in ordinary perception. He sees a house for instance, whereas previously he had seen only trees.)

To carry out a task in one's head is a way of avoiding carrying it out overtly. It is a substitute way of carrying it out; and as I have tried to explain earlier, if the full dress overt ways of carrying out a task are inhibited and one still insists on carrying out the task, then we have to rely upon various kinds of imaging and imagining in order to do so. Thus making these inhibitory movements is *a* logical condition of getting the imagery and imagining involved in carrying out tasks in our heads. By making these inhibitory movements then, the person gets imagery of many different kinds; and it would be just as fatuous to equate his imagery with the kinds of movements made, as it would be to equate talking with the mere movement of lips and tongue and the mere issuing of sounds.

'To have an image of A' is, as I have pointed out previously, an 'achievement expression'; and so are 'imagining that one is seeing X' or 'doing Y'. Since they are achievement expressions they do not refer to acts or processes, or to making certain movements, or to the movements made. Consequently to

observe the movements is not to observe what the person gets from making them, for what the person gets from making them is not the proper object of observation. What he gets from making them is what he makes use of in various ways and for many purposes in thinking, pondering, calculating, and the rest. As for those activities which go on in our heads but which are not task activities, they too will be logically unobservable and for the same reasons. They consist of images and often of imaginings as well, and we have to learn how to keep our daydreaming, reverie, nonsense and other trivia, to ourselves, instead of babbling it aloud as children do.

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VI.—DISCUSSIONS

THE IMPORTANCE OF PSYCHICAL RESEARCH

WHAT is the importance of psychical research? Psychic researchers, like many social psychologists, often speak as if experimental procedures and statistical methods guaranteed both the respectability and the real worth of their enterprise. There are non-scientific as well as scientific approaches to matters like agriculture, neuroses and the trade balance. The scientific approach to these matters has shown us the importance of soil fertilization, the significance of dreams, and subsidies, because we already knew the importance of sufficient food, happiness and general prosperity. The importance of psychical research, since it is now (unlike music, mathematics and philosophy) in the hinterland of our interests, will depend on how it can decisively be shown to bear on matters nearer the centre of our cultural map; *or* it will depend on a very radical change in our orientation. The bearing of psychical research on matters of acknowledged importance may have to be left an open question. But if we expect its influences to be more compelling than those of less recondite enterprises, we must be prepared for a radical change in orientation as well. Freud, *e.g.* could demonstrate, or did not need to demonstrate, how widespread and malignant were the symptoms of what he uncovered without inviting reorientation. Professor Broad in his recently published *Religion, Philosophy and Psychical Research* claims there is an analogy between our state of knowledge of psychic matters and that of, *e.g.* magnetism in the time of Gilbert. But we should have more trouble exploiting these alleged proclivities than magnetism. *Could* we all exercise these powers or *must* they be rare? Are we to interpret them by analogy with genius or with the secretions of the glands? There have always been records of people reputed to have powers not merely rare or extraordinary, but somehow weird and unorthodox. There are societies where such people are respected and feared, others where they are ignored or ridiculed, at the best tolerated. It is perhaps not too much to say that our way of life is of the latter kind. We classify people as sceptics or believers in the supernormal, but there are very many simply indifferent to these matters.

These trite observations may remind us that the importance of the psychic depends partly on how " ordinary educated plain men of Europe and North America " regard the psychic. A man is not rated intelligent, cultivated, profound, moral, sensitive or religious according to the interest he shows or fails to show in the psychic. Is a " common interest " in these matters like a common interest in chess or old furniture?

Broad speaks of " paranormal facts which have been established to the satisfaction of everyone who is familiar with the evidence and is not the victim of invincible prejudice " (p. 23). We usually

speak of prejudice for or against the facts when the facts are somehow in doubt. Are there not after all good reasons for resisting the conclusions which psychical research might suggest? It is not, as Broad suggests, simply a matter of either accommodating new facts or reforming our concepts to embrace them. For "reforming our concepts" is a task which we simply would not know how to begin. Nor would the evidence to hand seem to warrant such a revolution. Precognizers and clairvoyants compete with astrologers and fortune-tellers. They do not do mysteriously what ordinary people and scientists do palpably.

Part of the answer to the question of the importance of the psychic is to be found in the kind of impression it makes upon strong minds. Amongst these Broad is one convinced of the significance of at least some psychic phenomena. One essay in his book (*Immanuel Kant and Psychical Research*) is concerned with Kant's examination of Swedenborg's doctrines and of the philosophical problems of the psychic. Since Kant was pre-eminently one who kept his head when it was argued that it was only the facts, only what actually occurred, which made for knowledge, it is not surprising that his conclusions (as set out in his *Traume eines Geistersehers erlautert durch die Traume der Metaphysik*[1]) were, in Broad's words, "completely agnostic". In these matters we have "neither the guarantee of rational insight (as we sometimes have in pure mathematics) nor that of instantiation by sense-perception (as we have in natural science)" (p. 146). Broad remarks: "Kant never seems to have contemplated the possibility of an experimental investigation of ostensibly supernormal cognitive and active powers. . . . The fact that so great a man, speculating seriously on this topic, did not consider these possibilities, and that they have now been in so large a measure realized, should increase our gratitude and admiration for the pioneers of psychical research . . ." (p. 147). In Kant's day the issue was not complicated in that way, but I do not think we have the slightest ground for supposing that "scientific" investigations of these matters would have intimidated him any more than did the approach of his own day. In Broad's words, Kant found "the alleged phenomena . . . so unintelligible, and, even if genuine, so useless, that it is hard to accept them"[2] (p. 119).

Part of Broad's procedure is to examine his "Basic Limiting Principles" in the light of what he regards as the established facts of psychical research. One conclusion he reaches is that it may be necessary to abandon or overhaul the notion that an event cannot have effects until after it has happened. "For the occurrence of the cognition seems to be in part determined by an event which will not happen until *after* it has occurred: *e.g.* in Soal's experiments the subject's act of writing down the initial letter of the name of a certain animal seems in many cases to be in part determined by the

[1] Published anonymously in 1766.

[2] In a letter to Miss von Knobloch, probably written in 1763.

fact that the agent *will* a few seconds later be looking at a card on which that animal is depicted " (p. 19). Without resorting to these cases we can think of many which clearly involve, as this does not, both the notion of a cause and the notion of an effect, and which do seem to go against the necessity of the cause preceding the effect. A man attributes a rheumatic twinge to the approach of rain. He may be wrong to do so, but what he says is perfectly intelligible. If he is told that his twinge is due to the present state of the atmosphere, he may withdraw. But he might insist that we discover the state of the atmosphere by reading barometers and other instruments, and that a shower of rain is observed and a rheumatic twinge experienced. They are the kind of things between which causal connexions can exist. If there is room in our language for his way of putting it we shall not need the evidence of paranormal cognition to shake our belief in the necessity of a cause being prior to its effect.

But what *is* the event in Broad's example which is supposed to have effects before it happens? The phrase " the occurrence of the cognition " is invented to get over a difficulty. No sense has been given to it which allows us to say that " The cognition occurs " in any way resembles " The collision occurs " or " The twinge of rheumatism occurs ". And yet it would need to have such a sense before we could speak of it as an effect (like what happens on my retina) or as any kind of event. Nor is it at all clear that " The agent is or will be looking at the card " describes a state of affairs of the required ontological status. Broad typically interprets such expressions in terms of what he takes to be their natural counterparts; cognitions, perceptions, and perhaps even activities like looking, stand at the end of causal processes. He contrasts what might be an ordinary naive account of, *e.g.* what it is to see something with the sophisticated, scientific (causal) account of the workings of the visual apparatus. We ought, he thinks, to resist the temptation to say that we have direct acquaintance with physical objects by means of vision. For though seeing is " ostensibly prehensive . . . in its epistemological aspect, it is emanative in its physical aspect " (p. 31). It is by touching things and experiencing resistance in pushing and pulling that we come nearest to direct acquaintance with physical objects. Although it is logically possible that the function of the physical process " should be purely that of evoking and directing a cognitive act ", he thinks this suggestion " may quite safely be dismissed as inconsistent with the facts taken as a whole". But before we could dismiss the suggestion in this way we might first have to decide what it means.

If Broad does not think that there is a *logical* rivalry between " being able to see something " and " being able to give a causal account of vision ", and between " being able to point to a diagram of the visual apparatus " and " being able to point at what we see ", why does he reject the prehensive account as incompatible with the

empirical facts? For the prehensive account is not a causal account at all, even if it is a philosophical account of vision. Explaining why someone *cannot* see what is in front of his nose by detecting a flaw in his visual apparatus can be a very different kind of procedure from explaining that it is the perfection of the apparatus which enables someone else to see what is in front of his nose. Broad uses " directly apprehend " as if the causal accounts made it *unlikely* that we ever directly apprehend physical objects by means of sight or hearing. But the causal accounts have nothing to say about the status of " seeing ", " hearing ", " attending ", " observing ", " looking at ", " physical object ", and most important of all, " image ". What they do have something to say about is the status of " light-waves ", " sound-waves ", " particles " ," motion ", and what are here the most important concepts, " image *on the retina* " and " vibrations of the ear drum ". These things are certainly not in any way part of what we mean by " sights and sounds ", " images and noises ", let alone " physical object ". No one is taught to use these ordinary expressions via notions like " image on the retina ", " vibrations of the ear drum ". For someone to understand " image on the retina " and " vibrations of the ear drum " it is first necessary for him to have mastered our ordinary means for dealing with sights and sounds, etc. But not even in this respect do the use of words like " sensum ", " directly apprehend " and " existent " bear on the causal accounts. If Broad interpreted " sensum " as " image on the retina ", then it would at least to that extent be clear what he is doing. " Direct apprehension " might be that relation which a man has to the image on his retina. So it might be senseless to speak of M having that relation to the image on N's retina. But would it be senseless, would it not be merely unlikely that he should have that relation to another person's retinal image? It would of course be for Broad to say.

If we move from the familiar cases to those, *e.g.* where a man is said to correctly guess that the sixth card from the top of a new and mechanically shuffled pack is " the eight of Red Squares " (*Clairvoyance*, p. 37 *et seq.*), we are no longer referring to what anyone can do unless he has something wrong with him. Causal accounts of vision tell us what happens when things are going as they should. When they are not, we can in a particular case very often say what prevents a man from doing what we all do. In this respect or that his visual apparatus differs from ours. This makes it look as though a performance like clairvoyance requires *an extension of the ordinary models.* Broad puts up as a possible explanation of how the clairvoyant " gets his information " that his body is being stimulated by " emanations from the front of the card ". But why, asks Broad, should there be a special kind of emanation from that card and not similar emanations from the rest of the pack? The physical models for vision and the transmission of light provide causal connexions between object and observer. They are not

therefore causal explanations of observations or even perceptions. They provide answers to the question, " What happens when he sees the chair? " But we are in a different position with clairvoyance. We must of course distinguish those cases where the subject is ignorant of the nature of what has been concealed from those cases of extra-sensory perception where the subject might properly be said to be *guessing* (with whatever degree of success) at what is being concealed. If I say to someone, " Here is an ordinary pack of fifty-two playing cards. What is the sixth card from the top? ", then he will know that the reply " A cigarette case " is inappropriate. But if I say to someone without forewarning, " What am I holding in the palm of my hand? " and he replies, " A pin on the head of which is engraved the American Constitution ", then he can hardly be said to have guessed this, whether I am concealing such a pin or not.

Let us, however, consider merely the case where we ask someone at random which playing card is concealed from his view, and he answers correctly, " It's the Jack of Clubs ". We are then tempted to ask how it is that he is able to say it is the Jack of Clubs, seeing that he is in such a peculiar position with regard to the card. What bothers us is that his position is peculiar when we contrast it with the relative positions of an object and an observer, so peculiar in fact that to interpret his position as in any way a variant of the object-observer position deprives the latter of its distinctive character. Without referring at all to the physical models, we can refuse to consider clairvoyance as an alternative to normal perception. Nor does extra-sensory perception qualify as such an alternative. We shall only be tempted to make this step at all if the subject is at least nearly always able to say what is being concealed.

Suppose a clairvoyant were to admit that he sees the front of the card when confronted with its back. We might establish, *e.g.* by asking him to direct his attention the other way, that his field of vision somehow originates in the corner opposite to where he is standing. But no matter what he admits, we could only transform clairvoyance into a case of seeing by some such means as this.

There is the same objection to speaking of clairvoyance as a case of hearing, touching, tasting or smelling. It is easy to speak of a " sixth sense ", but what would it be to exercise it? Broad points out that the clairvoyant would need to be able to move from familiar cases of, *e.g.* seeing a card to the situation in which he is supposed to exercise his sixth sense. We learn to apply " That's a clock " in situations where we hear a clock ticking as well as in situations where we can see a clock which has stopped. But " That's the Jack of Clubs " is learned typically in visual situations alone. We might of course consider the case of a blind person being taught to use a pack of cards with raised pips. But his use of the expression would to that extent differ from our own. What would justify his use would be his learning to use the expression in a tactual

situation. Imagine a race of people who never claim to hear anything and who behave as if they never do, *e.g.* their talking is visual, a matter of lip-movements and gestures. We might be quite unable to say whether they are deaf or not. They may lack the concept of sound. But the clairvoyant is not one who has an experience for which he lacks a concept. The criteria for " I have an experience which you lack " have to be acceptable both to him who has and to him who lacks the experience. The clairvoyant uses expressions like " That's the Jack of Clubs " in situations directly opposed to those in which he learned them. For the clairvoyant must *not* be confronted with the card when he names it, while it is a necessary condition of teaching someone to use " That's the Jack of Clubs " that he should be confronted with the card. This is what makes being confronted with the card a paradigm case of being in a position to say " That's the Jack of Clubs ". In situations which do not obviously resemble this one we may be called upon to justify our use of " That's the Jack of Clubs ", even when it looks as though a claim that a certain card is the Jack of Clubs has been vindicated, *e.g.* where it is doubted that the claimant was in a position to make the claim.

The clairvoyant seems to be in a position where his claim is vindicated but his use of the words is not. It is just taken for granted that his claim is vindicated if, *e.g.* the sixth card from the top of the pack is the Jack of Clubs. If we interpret clairvoyance as just a most unusual case of being in a position to say something, then we shall not wonder if the clairvoyant's claim is really a claim at all. But perhaps we are merely being struck by a correspondence between the sense his words would normally have and a certain state of affairs, *e.g.* the sixth card does turn out to be the Jack of Clubs.

The case where a random answer coincides with what would otherwise be a true proposition provides us with a paradigm case for a coincidence. Broad speaks of " the odds against chance coincidence " being " billions to one " in referring to Dr. Soal's experiments on card guessing (p. 8). Certainly there will not be much point in going on saying " Coincidence " if a man says time and again what is being concealed in another man's hand. But an isolated case of coincidence is not like an isolated case of tuberculosis in a mansion, noted in a study of the relationship between tuberculosis and housing conditions. It is merely the frequency of what looks like some kind of success in clairvoyance which makes us say that there must be some explanation for the success. I am not allowed to go on saying that I picked the winner with a pin " by chance " if I pick winners with a pin at 179 race meetings out of 200. But what I *do* on 179 occasions is exactly what I do on the one occasion when I do pick the winner by chance; and exactly the same as what I do on the twenty-one occasions when I fail to pick the winner. The fact that I cannot foresee the future efficacy

of this method is of the greatest importance. Concepts like " chance coincidence " operate coextensively with concepts with " very seldom " and " sporadically ". But we ought to sharply distinguish the case of the stray bullet on the target from the case where we cannot say what it would be for the result to have been achieved by design.

A race of people, taught to say " That's X " in situations where we are taught to use such expressions, as well as in situations where we have learned that there is no reason for saying either " X " or " Not X ", will to this extent *seem* to make no distinction between being in a position to say something and not being in a position to say it. But this distinction they certainly will make. In using the expression in this way they will be guided by their own use of " That's X ". But nothing guides the clairvoyant in *his* use of " That's the Jack of Clubs ". On the contrary the normal use would here restrain him from saying " That's the Jack of Clubs ".

Clairvoyance provides us with a paradigm case for a mystery. Suppose someone asks, " But how can I say ' That's X ' when I am confronted with X? " One thing we might do, in an effort to bring out the utterly familiar, the perfectly ordinary and straightforward character of what puzzles him, is to ask him to imagine a case where a man names what is concealed from him without recourse to ratiocination and without being in a position to make a guess. In this way we might hope to transfer the feelings of puzzlement from the ordinary to the bizarre case.

The bizarre case may remain puzzling however we try to bring it in line with non-mysterious cases. We know how to answer questions like " What is that? " where we have been taught not the answer, but only how to make an answer to questions of this kind, *e.g.* where the question is accompanied by pointing to what is in full view. We only know how to answer questions like " What have I concealed in the palm of my hand? " in the context of a game. To play this game we must first enter into the spirit of it. This spirit is quickly exhausted if the game is not elaborated to allow for some other means to assess the value of the answers than the coincidence of any answer with any state of affairs. For why should not any answer do?

Broad rejects the analogy between clairvoyance and normal sense-perception, not because of these inherent difficulties, but because of the difficulty of constructing a causal model for clairvoyance which would be coherently analogous to the physical models for sight or hearing. These same physical models dominate his Basic Limiting Principles. Although these principles are supposed to be what we all accept, they are derived from a model for the mind-object relationship, which the physical models would seem to justify but which we have no good reason to accept as the simple literal truth about that relationship. Nor does this mean that such a model should be reformed to embrace further causal accounts of

psychic phenomena. There are enough facts to hand already to embarrass the epiphenomenalist and his opponents: no further facts will make such a view more or less plausible. Since it is not self-contradictory to speak of disembodied existence, we have already had good reason to doubt a necessary connexion between mind and body, to whatever extent "all other known facts", as Broad maintains, tended to support the epiphenomenalist view. So the establishment of paranormal cognition would not, as he maintains, be our only good reason for rejecting that view. In view of the inherent difficulties we might also doubt if anything has been established which "provides the basis for a more or less plausible explanation, in terms of established facts about the cognitive powers of embodied human minds, of phenomena which might otherwise seem to require the hypothesis of survival" (p. 26).

That a clairvoyant is in no position to say what card is being held up might tempt us to think that his none the less saying what card is being held up is an *effect* of something or other. But in the ordinary cases where he is confronted with the card his being able to name it is certainly not an effect of his being confronted with the card. Light-waves impinging on his retina in a certain way do constitute such an effect, but if his being in a position to name the card is an effect at all, it is an effect of his having been taught to name the card in similar situations. Of what is his naming the card, when he is in no position to do so, supposed to be an effect? We shall be much more inclined to say that it is an effect of something if we interpret looking, seeing and even saying in terms of what are thought to be their *natural counterparts, e.g.* events in the brain or elsewhere. We might interpret his saying "It's the Jack of Clubs" as an effect of his having been hypnotised and told to say "It's the Jack of Clubs", if we have some reason to believe that this has happened. But this use of "effect" will differ from the use in "light-waves impinging on his retina in a certain way constitute an *effect* of his being confronted with the card".

In considering telepathy (pp. 46-67) Broad denies that it constitutes a case of one mind's directly apprehending another. "It is enough to suppose that the occurrence of a certain sensation or imagination or bodily feeling in M's mind causally determines in N's mind the occurrence of a sensation with a similar sensum, or of an imagination with a similar image, or of a bodily feeling with a similar quality and feeling tone" (p. 54). But if we are to *causally* explain why each of two people has an image of X at a certain time, we shall first have to say what events stand in need of explanation. If M and N, as Broad suggests, arrange an "experiment" of this kind, then this arrangement will be made in such terms as "I am going to try to get a mental image across to you". And what this will suggest to anyone will be a matter of what he is prepared to make of it. Once again, as in the cases of extra-sensory perception and clairvoyance, we are asked to accept a correspondence (between

the sense an expression might have and a certain state of affairs) as sufficient demonstration that something unusual has been done or happened. But " I had a mental image of X at 4.32 " describes a state of affairs very different from " The sixth card from the top of the pack is the Jack of Clubs ". The use of expressions about mental images is not merely a rarified use of expressions about objects. We pick up talk about mental images and we learn to use expressions about objects. Although our use of expressions like " I have a mental image of an elephant " may very properly go unchallenged in everyday life, there is no reason why we should accept a particular use as descriptive of a *distinctive experience* like toothache. Just nothing like a paradigm case for " having a mental image " exists. It will hardly do to talk of " conducting experiments " with these things. Special instructions would also need to be given to a man on how to interpret " intense pains and a feeling of depression " as indicative of the plight of his friend, however vivid the mental images which accompanied his experiences. " It will be natural for N to assume that M must be very ill and perhaps dying " (*Spontaneous Telepathy*, p. 56). It will surely be more natural for N to assume that he himself is very ill and perhaps dying.

Broad interprets mental phenomena as kinds of objects; he interprets thinking as a process initiated by the will in the doubtful sense that the motor processes are initiated when I will my finger to move. In the latter case my moving my finger is the end result, what is desired, and more often than not achieved. But consider my wanting to think of a problem and being unable to do so. I know what it would be to want to raise my arm and yet be unable to do so, but " I can't think about the internal combustion engine " may simply mean " I don't know the principles of the internal combustion engine ". It certainly does not mean " Every time I try to think about it, I find myself thinking of the Taj Mahal ". Compare " Every time I try to raise my right arm, my left arm goes up ". " Failing to think about X " is most tyically " failing to think about X to any purpose ". But the results of deliberation are not what we set before us at the outset, for this would make deliberation superfluous. Broad tries to say, " We don't (can't) choose the order of our thoughts, except when we put them ' in order ' afterwards ". But this is highly misleading since it suggests that the way in which one thought follows another in a train of thought *must* be determined by some disposition of the thinker, or in some other respect lie outside the scope of his activity. Thoughts may follow thoughts in a certain order, or merely in that *sequence* which is part of what we mean by deliberation. No conclusion can be drawn from its being the case that this sequence does (must) exist in our thoughts which represents this sequence as " mental data " supplied from the thinker's storehouse of dispositions or past experiences. But if we do consider thoughts as data causally related to the previous experiences of the

person for whom they are data, we shall be able to speak of a thought which occurs in N's mind as having that relation to M's experiences which it normally has to N's experiences. This is Broad's pronouncement on telepathic discursive cognition and on the location of the natural counterparts to the dispositions. He sees no reason for placing them either in the mind or in the brain. But this, alas, does not mean that he has abandoned the notion of natural counterparts in this instance. "The modifications which are produced in this common substratum by M's experiences *normally* affect only the subsequent experiences of M; those which are produced in it by N's experiences *normally* affect only the subsequent experiences of N. But in certain cases this normal causal 'self-confinement', as we might call it, breaks down" (p. 67). But so far something like telepathy would be the only ground for such a speculation, and we shall only be tempted to speculate in this form if we find Broad's account of normal thinking acceptable. And we shall not do this unless we also find his theory of natural counterparts necessary to account for mental phenomena.

Naming concealed cards and comparing mental contents are without significance to people who teach their children to name what is *not* concealed and to distinguish between thoughts and "bare mental occurrences". If philosophers and psychical researchers combine in an attempt to convince these people that they are ignoring important facts, they are not likely to succeed in the attempt. One reason for this is that treating these cases as significant would involve a reassessment of the kind of inquiry into natural phenomena which we have learned to value by opposing the significant to the coincidental, the explicable to the baffling. If human beings had chosen a very long time ago other lines of possible development then those they actually did choose, the cases we call paranormal might occupy us today instead of those which do. But an alternative approach to the facts would require alternative means for dealing with them. Natural piety might restrain us from wanting things to be very different from what they are, even if the difficulties of reforming our approach to the world do not.

L. B. GRANT.

SLEEPING AND WAKING

I WISH to join Miss Macdonald (MIND, April 1953, pp. 202–215) and Mr. Baker (MIND, October 1954, pp. 539–543) in the controversy over the distinction between sleeping and waking. Mr. Baker maintains that Miss Macdonald's way of distinguishing between the concepts of dreaming and waking leaves undisturbed Descartes' assertion that "there exist no certain marks by which the state of waking can ever be distinguished from sleep". I shall try and put clearly what I take to be the main issue involved in this controversy. My particular aim is to establish the following two propositions:

(1) That a confusion of two different senses of the verb "to distinguish" and of two different levels of discourse lies at the basis of Descartes' assertion.

(2) That the issue between Mr. Baker and Miss Macdonald is to be settled on the basis of certain distinctions drawn under (1) and should therefore be decided in Miss Macdonald's favour.

The underlying idea of Descartes' predicament is briefly this: Since, when I am dreaming, I always "mistake" (take) [1] my dreams for real events (*i.e.* cannot *distinguish* between them), perhaps the events I experience in the waking state are likewise nothing but (are not *distinguished* by me from) dreams; therefore there are no certain marks by which we can ever *distinguish* waking from sleep. Here the verb "to distinguish" occurs repeatedly, and the question arises whether it has the same meaning throughout.

It seems clear to me that the first failure to distinguish—namely, that which consists in "mistaking" (taking) dreams for real events while one is asleep—is of a different sort from the other failure to distinguish, namely, that between waking and sleep. The first failure is not physical or psychological but in the same sense logical as our inability to apply tests in dreams. In other words, part of what we mean when we say that we dream is to say that during sleep we "mistake" (take) our experiences for waking experiences; it is precisely this fact which contributes to the logical unity and coherence of a dream. It follows that our failure in sleep to distinguish between dream happenings and waking happenings has no opposite: we cannot say that the failure is complete, partial, overcome or unjustified, nor that there are good, bad, or no reasons for it, nor that, though I and all the people I know are victims of this failure, there may be others who are not. Again it would be senseless to say that with a little more experience, or closer attention to this or that aspect of a dream, we could come to recognise in

[1] Here and elsewhere I have put quotation marks and an alternative expression in parenthesis to indicate that a dreamer is not deceived by, or mistaken about, his dreams in the ordinary sense of the word.

our sleep that our dream is a dream and distinct from waking experience. Just as from the point of view of the dreamer it is senseless to enquire what preceded his dream or to speculate what will come after it, so it is senseless to expect that the sleeper's inability to distinguish between waking and sleep is the sort of thing that can be improved upon or blamed. In fact, if we wish to pass from this state of total and permanent "confusion" to a state which allows the minimum of success in distinguishing between waking and sleep, we have no alternative but to wake up.

On the other hand, failure in the waking state to distinguish between waking and dreaming may be superseded by varying degrees of success in distinguishing, and the transition from failure to success takes place in one and the same state, *i.e.* the waking state, and it can be slow or quick, and supported by better or worse evidence; it may be an experience confined to one person or shared by many, and it may be the result of quick-wittedness or concentration. It is in this sense (and in this sense only) that Mr. Baker's twins [1] can, or cannot, be distinguished one from the other. My lack of discernment when I mistake one for the other is certainly very different from what happens when I "mistake" (take) a dream for a waking experience. In this latter case, when failing to distinguish between things as they appear to be and as they really are, I cannot strictly be said to lack powers of discrimination. Again, if I succeed in distinguishing one twin from the other, each becomes more of an individual reality for me; but the experience of a dream is dissolved once I know that my dreaming is not a waking experience.

I conclude that there are two widely different uses of the phrase "to distinguish" (or for that matter "to fail to distinguish"), and that it is fallacious to talk in the same breath of what is distinguishable (or indistinguishable) in one sense and what is so in the other. We would be trapped by the ambiguity of the expression, if we were to make inferences from one of its uses to the other, or to accept the word "distinguishable" in the one sense and the word "indistinguishable" in the other as a legitimate pair of opposites.

To return to Descartes' predicament. If analysed in the light of the previous discussion, it will be found to imply references to ways of distinguishing (or equally to failures to distinguish) at what may be called two logically different levels.

At the one level (I) there is the difference between the *concepts* of waking and dreaming, which Miss Macdonald has discussed in detail. Distinctions between the two concepts at this level are drawn explicitly for theoretical purposes; normally they are embedded in our experience. Possibly they are the result of our twofold activity in early life of learning to discern and to speak.

[1] Pp. 540, 541; I shall argue later that Mr. Baker's problem of how to distinguish between twins does not altogether correspond to the question under discussion.

There are at any rate a number of ways, more or less recognized, by which we distinguish dreaming from waking; for, as Professor Austin [1] has pointed out, how otherwise should we know how to use and to contrast the words? Descartes, as an afterthought, suggested one way of how to draw a distinction (*Meditation*, VI, last para.), and Hobbes suggested three others (*Leviathan*, ch. II), the chief one being that "waking I often observe the absurdity of dreams, but never dream of the absurdities of my waking thoughts". Hobbes' distinction is illuminating and almost identical with the one recently suggested by Mr. L. E. Thomas.[2]

At the other level (II) there is the practical problem of identification, *i.e.* the problem of ascertaining which *state* we are in at any particular time. We have to consider thus the case of waking (X) and, separately, that of sleep (Y). In the case of X there is no difficulty, since we can recognize that we are awake whenever we are in a position to apply Hobbes' criterion (as one among others). For it is only in the waking state that we can compare our present waking thoughts with our past dreams and observe the absurdity of the latter, their lack of continuity and coherence, and so on; in dreams this dichotomy disappears. In the case of Y, on the other hand, it is not only difficult but impossible ever to achieve this task of identification at any particular time of dreaming. As Hobbes himself put it: "being awake, I know I dream not; though when I dream I think myself awake". My failure to identify Y while dreaming is that *in a sense* I fail to distinguish Y from X, being enveloped in *one* unitary and coherent experience from which the contrast between X and Y is absent. But my failure to distinguish Y from X in this case is unlike any failure to distinguish Y from X which I might admit at level I, and it is not the opposite of any success I might have in distinguishing Y from X at level I.

Paradoxes and other curious problems generally arise because of the failure to distinguish between levels I and II.[3] If these levels are not distinguished, there is at all events a tendency to maintain that it is very difficult if not impossible to distinguish waking from sleep. Descartes' predicament arises precisely because for him the fact that in our sleep we "mistake" (take) dream happenings for waking happenings is sufficient justification for the assumption that what we take to be waking events are nothing but dream events, and that therefore there is no certain way of distinguishing between the waking state and sleep. In this inference he passes illegitimately from what I call level II to level I. It should be clear now that there are two steps in Descartes' argument, each one of which is

[1] "Other Minds", *Proc. Arist. Soc.*, Supp. vol. xx, 1946; reprinted in *Logic and Language* (2nd ser.), 1953, p. 133.

[2] "Waking and Dreaming", *Analysis*, June 1953, p. 127.

[3] C.f. my paper "Descartes and Hobbes on Waking and Dreaming", in *Rev. Int. de Philosophie*, 1956.

concerned with a failure to distinguish between waking and sleep; that, since these two failures have been shown to be logically different, there can be no valid inference from the one to the other.

To come now to the issue between Miss Macdonald and Mr. Baker. To my mind this arises from Miss Macdonald discussing the logic of the distinction between waking and dream experience at level I, and Mr. Baker attempting this at level II, each claiming that his or her kind of discussion alone can lead to a refutation of Descartes' lament that "there exist no certain marks by which the state of waking may be distinguished from sleep".

The significance of Miss Macdonald's discussion is that she makes explicit certain fundamental distinctions between the concepts of waking and dreaming at level I, thereby in fact taking away whatever force Descartes' lament may have. For since this is caused by the assumption that the distinctions at level I can be obliterated by the failure to distinguish between waking and dream life at level II, any enforcement of the distinctions at level I destroys the justification for Descartes' lament. Now Mr. Baker's point is that the distinctions drawn by Miss Macdonald are of no help in deciding whether one is awake or dreaming at any particular moment, and that she ought to have concerned herself with issues arising at what I call level II. I want to argue that both these charges fall to the ground.[1]

In the first place, Miss Macdonald's criteria for distinguishing waking life from dreams should help, though not perhaps as directly as Hobbes' criterion, in determining that I am awake now, for they can be applied in the context of waking experience and in that context only. In any case, if I am to ask and answer meaningfully such a question as "Am I awake or asleep?", the distinction between what is waking experience and what is merely a dream must be known to me along such lines as Miss Macdonald suggests. Her criteria cannot of course be used to determine that I am dreaming now: nothing can ever determine this. Miss Macdonald is doubtless right with her recommendation that philosophers should not overlook the peculiar significance of the verb "to dream" and should pay attention to the logical status of the main clause in such sentences as "I *dreamt* that I perceived, did, chose, etc.". But she would agree, I think, that not even a philosopher can help overlooking in his dreams the significant use of the word "dream", and that as long as he is dreaming he has no use whatever for the main clause in the above sentence, as *all* his experience is of the form "I perceive, do, choose, etc.", rather than "I dream, I perceive, etc."

[1] I think that my arguments in defence of Miss Macdonald can also serve in reply to the article written by R. M. Yost, Jr. and Donald Kalish ("Miss Macdonald on Sleeping and Waking", *Philosophical Quarterly*, April 1955) which was published after I had completed this note.

In the second place, I think Miss Macdonald is right in showing no concern in issues that arise at what I call level II, precisely because Descartes' lament cannot be refuted by any argument derived from experiences at that level. Mr. Baker's concern is to determine, from "certain distinguishing marks of appearance", which state I am in when I am wondering whether I am awake or dreaming, and he believes that if Descartes' lament is to be refuted this can be done only on the basis of such distinguishing marks. What I find difficult to understand is Mr. Baker's contention that if we are to distinguish *states* of dreaming and waking we have to find distinguishing marks in their appearances *in the same way as* we are dependent on such marks of appearance if we wish to know in the case of twin sisters who are very much alike whether we are in the company of Alice or Anne. The parallel between the waking state and sleep on the one hand and the resembling twins on the other is surely misleading, for if my meeting Anne is to correspond with my having a dream experience, then I shall *never* be in a position to address Anne as Anne, regardless of her appearance and whatever else I may know about her. So far as the waking state is concerned, I think that, if in most cases we have no difficulty in identifying it, the reason is that we can apply the criteria for distinguishing between waking and sleep at level I (*e.g.* Hobbes' criterion, or Miss Macdonald's criteria) whenever we are awake, and only when we are awake. On the other hand, I don't think we are ever in a position to *prove* (*i.e.* conclusively demonstrate) that we are awake now, for neither reasoning nor tests are adequate to the task: reasoning cannot establish matters of fact, and when we rub our eyes or pinch ourselves in order to determine that we are awake, it is always possible that we merely dream that we perform these actions. What is not possible, however (and this is the important point in my view), is that the inappropriateness of tests and, generally, the absence of proof in these matters can in any way affect the distinctions we draw between waking and dreaming at level I. For, as I have tried to show, if in this or that case I am in fact dreaming or may possibly be dreaming though I think myself awake, this failure to distinguish between the two states at level II is different from a failure to distinguish between them at level I and cannot therefore by itself obliterate our distinctions at that level. The only procedure, then—and I consider it an entirely adequate procedure—for refuting Descartes' lament is to emphasize our distinctions at level I, and this is precisely what Miss Macdonald has tried to do.

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In her contribution to the Symposium on ' Thinking and Language ' Miss Murdoch rejects the view that thought is the uttering of mental words ; " words do not occur as the content of thought as if they were cast upon a screen and there read off by the thinker. If we explicitly imagine the uttering of a verbal message to ourselves this contrasts with the more confused way in which words do occur ' in our minds '. . . . The meaning character of uttered speech often demands an awareness of gesture, tone, and so on, as well as of context, for its full understanding. This is clearly so too, mutatis mutandis, for inward ' speech '. The thought is not the words (if any) but the words occurring in a certain way with as it were, a certain force and colour."

If Smith tells me that his son is a liar, I may know how serious it is from his manner. But suppose Smith says to himself, ' My son is a liar.' Does *he* know that it's serious from the tone ? Of course he might, as when we are taken by surprise ; ' I was shocked at my own bitterness '. But in these cases we treat ourselves as a third person. When we talk to friends, particularly to intimate friends, our speech becomes sketchy and is yet understood. If one can do that with intimates, what cannot one do with oneself ? How often when we are explaining something to a friend are we interrupted by —' Yes, yes, I know !' How is it that we do not bore ourselves ? Miss Murdoch asks us to imagine the uttering of a verbal message to ourselves, and to contrast this " with the more confused way in which words do occur ' in our minds ' ". Words do not occur in our minds in a confused way. We suppose them to be confused if we fail to notice that words perform a different function when spoken to oneself from when spoken even to a friend, as a question or a command to oneself is different from a question or a command to another. We take them to be confused if we take them to express the thought, or to be part of the expression of the thought, or if we take them to be cogs in a machine which does the thinking. They are very commonly used by us in thinking, but not in a specifically linguistic way. They are not instruments in the way that a hammer is an instrument where the weight and shape determine the use, nor materials as a child's blocks are materials whose use is fixed by their size and shape.

Miss Murdoch goes on, " It may be true both that we do not learn mental words in connexion with inner experience, and also that we verify, or justify, propositions which contain these words (when they refer to other people) by reference to conduct. It does not follow from this that there is ' no such thing ' as the inner experience or that it is not (in some cases at any rate) what is meant by the mental words, in a perfectly familiar sense, of ' meant '." It is certainly true that we do judge whether or not another person does intend to

do what he says he will do by his manner, by what we know about him and others, and by what he does. It is just as certainly true that we do not, in our own case ask the question in the same sense. When we do ask the question it is as equivalent to ' Do I really intend to do X ?', a third person question or, ' Shall I do X ?', not a request for information but the approach to a decision. Let us suppose the decision made. Jones who has been dithering for some time about whether he will accompany his wife to the beach says, ' Have you put my things in the car ?' or ' I'll come ' or, ' I've decided to come '. The last would be appropriate only if there are considerations which have been gone over or weighed. We can imagine considerations being gone over silently or aloud ; but to ' weighing up ' the terms ' silently ' and ' aloud ' are not applicable. Yet there are cases where it would be immaterial which expression one used.

It is sometimes suggested that the meaninglessness of such sentences as ' Do you know that you have decided ?' implies that decisions are part of our inner experience. But it is the word ' experience ' not the word ' inner ' which entails that such sentences are meaningless, and the entailment would vanish if we were to speak of inner life instead of inner experience. If a decision were part of our outer experience, these sentences would still be meaningless. ' I promise to come ', ' I've decided to come ', ' I will come ', ' I intend to come ' parallel ' I can assure you ', ' I know ', ' I believe ', and ' I think '. If Jones says ' I'll come ', he commits himself ; if he says, ' I'd like very much to come ', he commits himself. It is the absence of this element that makes ' unconscious decisions ' and ' unconscious desires ' different from their conscious counterparts. They do not commit their owners. We think of desires and decisions as primarily private and as made public. We should do better to think of them as primarily public. When we make a decision of which we tell no one, our model is the usual public decision. We keep faith with ourselves. When Jones says ' I'll come ' he does not inform his wife of what has gone on in a world from which she is excluded ; nor does he make a prediction about what she may expect (one cannot predict what it is in one's power to bring about) ; he gives an undertaking, or less strongly lets people know where they stand. ' I intend to come ' limits the degree of commitment.

" It may be said," writes Miss Murdoch, " why should we want to characterise an individual thought ? Why should we regard it as a mental datum ? What we look at when using mental words is context and conduct, not inner events. This is true up to a point. . . . But in fact, to us (as opposed to the external observer naming our goings on), our imagined monologues are not always unimportant, and we *do* attempt to characterise particular events which occur in them. In Daniel Deronda when Gwendolen hesitates to throw the life belt to her husband, who subsequently drowns, it matters very much to her to know whether or not at that moment, she intended his death." Gwendolen admits to having killed him ' in

her thoughts', but if she had thrown the rope, we would not take that seriously. No doubt we would think her to be in a very queer state. But we would not suppose that she intended to kill her husband. It is not suggested that her failure to throw the rope was due to clumsiness. If she is to be excused we must suppose that she lost her head or that she was out of her mind. What is Gwendolen puzzled about? Does she want to know whether she was in her right mind? Or is her problem a problem in casuistry? Is a person who fails to help one in danger of death guilty of murder? In giving judgment, Lord Porter said "Prima facie, a man who treated his wife with gross brutality might be presumed to intend the consequences of his acts. Such an inference might indeed be rebutted. . . ."[1] Gwendolen failed to throw the rope. Unless she was out of her mind at the time, she must have known the consequences of her action. Unless other evidence of a contrary tendency can be found rebutting the charge we must presume that she intended his death. Miss Murdoch agrees that if the matter had come before a court it would have been dealt with thus, but, she says "To us (as opposed to the external observer naming our goings on), our imagined monologues are not always unimportant", At his trial when Roskolikov was asked for his motive "he answered very clearly with the coarsest frankness that the cause was his miserable position, his poverty and helplessness, and his desire to provide for his first steps in life by the help of the three thousand roubles he had reckoned on finding. To the question what led him to confess, he answered that it was his heartfelt repentance. All this was almost coarse. . . ." Like Miss Murdoch Dostoyevsky suggests that clarity and coarseness go together. This is no doubt true. What is in question is whether the interminable soul-searchings which can mark the examination of one's own conduct and the conduct of intimates involves a sort of knowledge of oneself (and others?) different in kind from the sort that coarsely enough emerges in the court room.

Miss Murdoch speaks as though imagined monologues were real monologues uttered, as we say, beneath the breath. But there is no evidence that Gwendolen said anything either aloud or beneath her breath at the time of her husband's drowning. She says to Deronda, "I know nothing. . . . I saw my wish outside me . . . and I held my hand, and my heart said, 'Die!' . . . and he sank; and I felt 'It is done—I am wicked, I am lost!'" There is nothing here to suggest an internal monologue at the time, and not much to suggest that she was unaware of the consequence of her action. The expression "My heart said 'Die!'" is used by her to deny responsibility. Her heart is not Gwendolen, as our repressed desires are not our real desires. We may compare expressions of this sort with formally similar expressions, 'Reason compels me', 'Conscience

[1] In Lang *v.* Lang before the Privy Council.

tells me ' and with the language of possession—' Something got into me ', ' You must have been possessed '.

Professor Ryle and Miss Murdoch agree that we can say what our musings are like but only what they are like. Miss Murdoch thinks that this is all for the best. " In such a context metaphor is not an inexact faute de mieux mode of expression, it is the best possible." Professor Ryle offers a reason why we lack words to report unpicturesquely on the " concrete incidents of a stretch of thinking ". We would like, he says, to break down thinking into its ' ingredient processes '. This is a misguided desire. Thinking is, like work, a polymorphous concept. In the same context Professor Ryle suggests that " our ordinary ways of describing our ponderings and musings tend to be graphic and not literal . . . because we want them to be histories and we want them not to be chronicles ". If thinking is an activity, it is a pointed activity, and perhaps also it is polymorphous. But I find it difficult to understand why our disappointment in the search of a common character should thrust us into metaphor. There are other polymorphous concepts which do not have this effect.

I want to distinguish two linguistic moves. One of these is the use of one or more of a set of terms which have an established system of entailments, in circumstances which make nonsense of some of the entailments. Examples are the use of ' high ' and ' low ' of sounds and of rank. We speak of a sharp pain and a dull pain. We fail to grasp an argument or see a point. Our thoughts go round in circles, and we get lost in a fog. " I can, indeed *tell* you what my musings were like " says Professor Ryle. My pain is not like a sharp knife. It is sharp in the way that pains are sharp. I am not like one lost in a fog. I am lost in a fog. We are tempted to say, not a real fog, but to do so is to be dense. It is as though one were seriously to point out that the high notes of the piano are not really any higher than the low ones. This confusion is linked with the mistake of supposing that some words are descriptive. Except in the sense that they may occur in a description, there are no descriptive words. Having a word for a thing is not having a description of it ; so having a word does not remove the mystery. The sharp pain, the failure to hold the thread of the argument, the casting about for an illustration, are not even dead metaphors. Metaphors throw light. There never was a time when talk about the thread of an argument, or the sharpness of a pain, or the lowness of a note threw light on the argument or the pain or the note. ' Chemise de cylindre ' is no more a metaphor than ' cylinder liner ' or ' clothes horse '. The only advantage that the term ' cylinder liner ' has over a catalogue number is that, in a context, one can guess its application. A metaphor throws light on its subject, not on what the subject is. ' I did it in my head ', ' I'll keep it in mind ', ' Have you weighed up the evidence ?' ' Please give it your attention ' are not and never have been metaphors though they were once fresh as daisies.

Miss Murdoch claims "that thinking is a private activity which goes on in our heads, that it is a content of consciousness". This vacillation between the language of activity and state runs throughout her paper—on the one hand 'inner event', "mental event", "Mental activity", on the other "content of conciousness", "images", "state of mind", and then her frequent recourse to words which avoid the issue, as "mental life", "inner life", "mental experience", "inner experience", "imagined monologues". Professor Ryle has pointed out that if thinking were an activity one would expect to be able to ask such questions as 'How long have you been thinking?' but in fact one can ask only 'How long have you been thinking about X?', and even that is not straight forward. For one cannot ask a person to stop thinking about X as one can ask him to stop talking about X. And this is not because such a command is difficult to enforce. A command to cease talking might be difficult to enforce. It is more like a command to stop breathing and still more like a command to stop living. This is what has made some say that the mind thinks always, and that serious thinking is to be distinguished from just thinking as serious living is distinguished fron just living, by having point. But we must not let this analogy persuade us that there is a sort of thinking that is pointless.

Suppose I ask Jones whether Smith would be a satisfactory candidate for a post. Jones says, 'I'll think about it'. Next day I see Jones who says, 'I think Smith should do very well'. Can I properly ask how much time he devoted to thinking about Smith? Suppose Jones says, 'I don't think his name crossed my mind'; does it follow that his answer was unconsidered, or even that he has not thought about Smith? Might he not as easily have said, 'I had it at the back of my mind all day' or, 'If there had been any objection to Smith I would have thought of it'. We should not let the expression 'I would have thought of it' delude us into concluding that in fact he did not think of Smith. Thinking is more like paying attention and looking than finding, but we do in part judge by results even where the thinking or the looking is unsuccessful. If we have looked or thought, we must be able to point to some moves which are no longer necessary. It is because there are moves that we can order a person to think about a problem or to look for something. Probably our first experience of the word 'thinking' comes in learning to keep our thoughts to ourselves, and in learning not to act precipitately. We are told 'Think before you speak'; 'Stop and think'. We learn to hide our feelings, our opinions and our motives, and we learn to wait. We learn that there is more to hiding our feelings and motives than refusing access to our inner selves, that in fact people accept very little of what we tell them of these things, preferring to rely on our actions, our faces, and perhaps a turn of expression which slipped out and was not what we meant at all.

When someone thinks or dreams or does nothing in such a way as to give us no clue to what he is doing (what point there is in this silence, not what changes are taking place), when as we say he retires into himself, we speak of an inner life from which we are excluded. Part of this innerness is simple secrecy. Part consists in his being able to do or not to do what he chooses, in his actions being his not ours; in his being responsible. Compare 'What is he going to do? I don't know, I have not read the programme' with 'What is he going to do? I don't know, I can't see inside his head' and 'Where is Jones? How should I know? I'm not his keeper.' This point applies more generally than might be suspected since we are inclined not to notice that being sad or angry is not just something that we notice about ourselves, but something that we have a say in. When we close our eyes and our ears and try to remember something, this too seems inner in that we seem to turn away from a common world into a world of our own, to which others have no access. For though they remember what I remember their memories are not mine. Here too the claim to special access turns out to be an oddly couched reminder that you are not me.

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At least some of the difficulty that is generally encountered in considering this principle arises, I think, from an element of ambiguity in the word "distinguish". For the purposes of showing how this is so, I shall use the word "quality" in a sense such that relational properties are not qualities. Using the word in this sense one might be inclined to accept the following argument in refutation of the principle:

Two objects cannot be alike in all qualities without being two objects. Therefore, however alike they may be, they cannot be so alike as to be no longer two. Therefore the statement that different objects are never indistinguishable [1] is a mere tautology.

To put the argument in another way, to say that there are no two objects that are indistinguishable tells us nothing, since to say that there are two objects is already to have distinguished them. That is to say, they have already been distinguished by that element in the situation in virtue of which they are called "two objects", namely, a relationship (usually spatial). In short the argument amounts to saying that two objects can be distinguished, even if they are alike in all qualities because, being two, they are terms of different spatial relations. Now whatever Leibniz may have meant by the principle I do not think that by this argument he can be shown merely to have expressed a tautology.

The point I wish to make can be made more clearly with the help of the following illustration. Consider a world consisting of these three objects only: two black billiard balls and an observer. Let the observer be placed exactly half-way between the two balls and in line with them. He can, so situated, name the ball nearest his one hand, "A", and the other "B"; that is to say he can, to this extent, distinguish them. If, now, he were blindfolded and spun round for a few seconds, he would not, on removing the blindfold, be able to say which is the ball "A" and which "B". He could of course still distinguish them as before, although he now knows that any names he may give them will be vacuous. In one sense of the word "distinguish", then, he can distinguish them, but in a stronger sense of the word he cannot. The two senses are:

(1) Point out, count or enumerate on a given occasion.
(2) Identify on different occasions.

Interpolating these two senses of the word in the above argument, it is clear that (*a*) from the fact that two objects, though exactly alike, are nevertheless terms of different spatial relations, it follows that they can be pointed out, counted or enumerated on a given occasion, but that (*b*) from the same fact it does not follow that they can be identified on different occasions.

[1] Leibniz, Nouveaux Essais, II, 25 iii.

The imaginary observer cannot identify the balls on different occasions because his situation has been deliberately constructed to prevent him doing so. The situation excludes all means of establishing a set of co-ordinates by which the different spatial relations between the three objects in that world could be determined. If, for example, each of them had been placed at the corners of a triangle, instead of being placed in a straight line, the observer would have had no difficulty in distinguishing between the balls after he had been spun. But in using phrases like " two objects " and " halfway ", it has been assumed in the description of the situation that there is a set of co-ordinates by which the different spatial relations between the three objects could be determined. For, by objects " we mean, usually, " things occupying two (at least two in principle) determinable positions in space ". So what we have done is to imagine a world in which there are just two objects besides an observer, all three being so related that the observer cannot in practice determine their spatial relations. What we cannot do, is to imagine a world in which it is in principle impossible to determine any spatial positions in it. We could not even describe such a world, since it would be merely a void to which the phrase " spatial position " would have no application.

But, although we are compelled (logically) if we speak of two things, to conceive their occupying two determinable positions in space, it is clear from the illustration that we are not, at the same time, compelled (logically) to conceive their *distinguishably* occupying those positions in space, in the stronger sense of the word " distinguishable ". Since we can, then, conceive a situation in which two objects that are alike are not distinguishable, in one sense of the word, it is necessary to qualify the above argument. For, although in one sense of the word " distinguishable " the argument clearly does (given the distinction between " quality " and " relational property ") show the principle to be a mere tautology, in the other sense of the word it does not. Using the word in the stronger sense, the premise of the argument, although true, leads to the false conclusion that the statement: " Different objects can never fail to be identifiable on different occasions " is a tautology.

It may be objected that there is no need to go to the trouble of conceiving, in this fanciful way, a situation in which two objects that are alike are not distinguishable, because only objects that are alike could conceivably be indistinguishable, and therefore *any* objects that are alike could conceivably not be distinguishable (in the stronger sense). The plausibility of this objection, however, rests on the use of the word " distinguish " in a third sense, perhaps the most common of the three, namely in the sense of *easily* identifying on different occasions. For, while it is certainly true that only objects that are alike could conceivably be difficult to identify, it does not follow that any two objects that are alike could conceivably be indistinguishable, in the sense of being completely

unidentifiable on different occasions. This has to be shown, hence the illustration.

My conclusion is simply that arguments for or against the principle of the identity of indiscernibles, which, like the one considered, turn on the sense of the word "distinguish", only add to the confusion if the sense of the word is not specified. The confusion is already there in Leibniz' own statement of the principle, for, on reading it, we are tempted to ask: "Is he saying that two things which cannot be distinguished are not really two, but one?" and then: "But how could we start to try and distinguish unless we already had more than one object in front of us; we can fail to distinguish only if there is not more than one object there from the start!" and so on. It would, I think, be a step towards clarity to specify a sense for the word "distinguish" before using it in this context.

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A NOTE ON AN ARGUMENT OF QUINE

QUINE has rightly called attention [1] to a difficulty standing in the way of consistent interpretation of *Principia Mathematica*. It appears also to be suggested that a like difficulty stands in the way of consistent interpretation of any logical system which utilizes both property abstraction and class abstraction. It certainly behoves the logician, who thinks that the logic of intensions and the logic of extensions may be united in a single comprehensive theory, to attend carefully to Quine's argument.

In my paper, "Modality and Description",[2] I attempted to show that certain paradoxes due to Frege and Quine, which threaten the interpretability of modal logic as a theory of deduction, may be averted by considerations pertaining to the scopes of definite descriptions and class abstracts. I wish to apply those same considerations to the problem created by Quine's present argument. Though the logic of the situation is similar to the case of Frege's well-known example of the morning star, there is this important difference. The present argument does not turn on the explicit admission of modal operators into logic but only on the recognition of the diversity between classes and attributes.

In Quine's present discussion it is correctly pointed out that if the rules of *Principia* were supplemented by the permission to infer identity of attributes from their logical equivalence, a paradoxical result would be achievable, namely, the result that all formally equivalent attributes would be identical. It is clear, however, from other considerations than Quine adduces, that this supplementary principle is not admissible in *Principia* as it stands. *Principia* utilizes the so-called ramified theory of logical types according to which logically equivalent propositional functions may not always be identified. For example, if ϕ is a predicative function (or attribute) the following propositional functions must, according to the ramified theory, be distinguished, though they are logically equivalent:

$$\phi\hat{x}$$

and

$$(\exists\psi)(\psi!\hat{x}\,.\,\psi=\phi).$$

Thus, the supplementary rule of inference suggested by Quine does not accord with the interpretation provided by the authors of *Principia*. This circumstance, I agree, must raise a question as to

[1] W. V. Quine, "On Frege's Way out," MIND, vol. lxiv, No. 254, pp. 145-159. This discussion pertains exclusively to the appendix, pp. 158-159.

[2] *The Journal of Symbolic Logic*, vol. 13, No. 1, pp. 31-37. See also Quine, "The Problem of Interpreting Modal Logic", *Journal of Symbolic Logic*, vol. 12. No. 2, pp. 43-48. My review of this article is in vol. 12, No. 4, pp. 139-141. Cf. Fitch, F. B., "The Problem of the Morning Star and the Evening Star", *Philosophy of Science*, vol. 16, No. 2, p. 137.

what can be meant by an attribute in that system. Nevertheless, one can provide a new interpretation of the symbolism of *Principia* utilizing the simple rule of types but altering the *Principia* definition of class abstracts in an important respect, presently to be indicated. The purpose of this is to show that it is possible, so far as is known today, to develop a comprehensive theory unifying the logic of intensions and the logic of extensions. According to the interpretation presently to be suggested, one is able consistently to use the supplementary rule of inference suggested by Quine, which authorizes us to infer identity of attributes from their logical equivalence. In other words, Quine's argument will not apply to the system presently to be outlined.

Let us assume, for the sake of argument, that properties are of two sorts: properties in extension and properties in intension. Properties in extension are to be identified if they have the same instances. Also, every property whatever will be assumed to be formally equivalent to some property in extension. For brevity we shall sometimes say "extensional property" in place of "property in extension". Similarly, we shall sometimes say "intensional property" instead of "property in intension". We shall reinterpret the *Principia* symbol, "$\phi ! x$" to mean the same as "x has the extensional property ϕ". We summarize these assumptions by introducing two axioms:

$$\text{A. } (\exists\psi)\,(\psi ! x \equiv_x \phi x).$$

$$\text{B. } \phi ! x \equiv_x \psi ! x \supset \phi = \psi.$$

The present system differs from *Principia* in that it formally differentiates, by means of B, between intensional and extensional attributes. In the same spirit, we could, if we liked, also postulate

$$\text{C. } (\exists\phi)\,(\exists\psi)\,(\phi x \equiv_x \psi x \,.\, \phi \neq \psi).$$

The axiom of reducibility and the ramified theory of types is here abandoned but instead we make the stronger assumption of classes, in the form of the postulate, A.

Also, instead of using the *Principia* symbol, "$\psi\hat{x}$" for purposes of functional abstraction, let us use "$\hat{x}(\psi x)$" for the same purpose. For example, we use "$\hat{x}(x$ is blue)" to designate the abstract condition expressed by the formula "x is blue." In order to introduce the symbol for *class* abstraction we shall utilize the *Principia* definition in use, except that we shall have to acknowledge that the scope of the abstract may in certain situations be relevant to the truth value of the context in which the abstract is embedded. We therefore adopt the definition:

$$[\hat{x}(\phi x)]\,.\,f\hat{x}(\phi x)\,. =_{df} (\exists\psi)(\phi x \equiv_x \psi ! x : f(\psi)).$$

We shall, in general, follow the convention employed in the theory of definite descriptions, of omitting the scope indicator when the scope of the abstract is the shortest formula containing the abstract.

Be it noted that the authors of *Principia* show, on page 188, that the class abstract has a scope though they do not appear to appreciate the importance of this fact.

Now, if this system is supplemented by the rule of inference suggested by Quine

C. If $\vdash \phi x \equiv x\psi$ then $\vdash \phi = \psi$

we obtain no untoward consequences, as I mean to show.

First, it is provable that

D. $\phi x \equiv x \epsilon \hat{x}(\phi x)$

from which we infer

E. $\phi = \hat{x}(x \epsilon \hat{x}(\phi x))$

which ,because of the convention for omitting scope indicators, is

E′. $\phi = \hat{x}(\exists \psi)(\psi ! x \equiv_x \phi x : \psi ! x)$.

This last proposition is not paradoxical. It says that any property, ϕ, is the property which an object has just in case it has some extensional property formally equivalent to ϕ. It should be noted that had we taken the scope of the class abstract to be the whole of E, we should have obtained, instead of E′,

F. $(\exists \psi)(\psi ! x \equiv_x \phi x : \phi = \psi)$,

which says that any property is an extensional property or that a property is the same as the class which it ordinarily is said to determine.

I submit that no reason has been presented which requires acceptance of F. Let us consider Quine's argument as applied to the revised system outlined above.

We would have to grant Quine's (25), (26) and (27) with the proviso that the scopes of the abstracts are the shortest formulae containing them. That is, we accept the supplementary principle, C above, together with E and its variant, $\psi = \hat{x}(x \epsilon \hat{x}(\psi x))$.

Now, Quine's argument utilizes * 20.18 of *Principia* which is "$\hat{x}(\phi x) = \hat{z}(\psi z) \supset . f\hat{x}(\phi x) \equiv f\hat{z}(\psi z)$." Attention to the derivation of this formula shows that the scopes of the abstracts must be no smaller than "$f\hat{x}(\phi x)$" and "$f\hat{z}(\psi z)$" respectively. Application of * 20.18 yields, therefore,

$$\hat{z}(\phi z) = \hat{z}(\psi z) \supset : [\hat{z}(\phi z)] . \hat{x}(x \epsilon \hat{z}(\phi z) = \phi . \equiv [\hat{z}(\psi z)] . \hat{x}(x \epsilon \hat{z}(\psi z) = \phi.$$

However, to obtain the desired conclusion, viz.

$$\hat{z}(\phi z) = \hat{z}(\psi z) \supset [\hat{z}(\psi z)] . \hat{x}(x \epsilon \hat{z}(\psi z) = \phi.$$

we need to be able to assert

$$[\hat{z}(\phi z)] . \hat{x}(x \epsilon \hat{z}(\phi z)) = \phi.$$

which is just what we lack authorization to do. We have already seen that this last formula does not follow from E above.

It should be noted that Quine's argument will have no pertinence to a system of logic unless it is provable in that system that the scopes of class abstracts cannot affect the truth values of the contexts in which they are embedded. It can indeed be proved that:

$$p \equiv q \supset_{p,q} f_p \equiv f_q \supset : f\{[\hat{x}(\phi x)] \,.\, \lambda\hat{x}(\phi x) \,.\,\} \equiv [\hat{x}(\phi x)] \,.\, f\{\lambda\hat{x}(\phi x)\}.$$

But to urge that every context, f, satisfies the condition,

$$p \equiv q \supset_{p,q} \cdot f_p \equiv f_q$$

would be to beg the question. In particular, a context of the form,

$$\text{H.}\quad \hat{x}(x \epsilon \hat{x}(\phi x)) = \chi,$$

is not such that substitution on "ϕx" of *any* formally equivalent formula, say "ψx", would leave the truth value of H unaffected.

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ON EVIL AND OMNIPOTENCE

GOD is omnipotent. God is wholly good. Evil exists. Professor Mackie argues (MIND, April 1955) that if any two of these propositions are true the third is false. His argument either virtually takes for granted the moral sufficiency of a free will that is compatible with determinism; or it demands logical impossibilities from omnipotence; or, just conceivably, it rests on an unnoticed claim to know what possible free agents would do in possible worlds.

Mackie asserts (p. 202) that if we really understand the three propositions, we must see the futility of all attempts to defend their mutual consistency without changing their meaning. As he goes on, however, he seems to change his mind about this and to regard as necessarily fatal to the perfection of God's goodness or power not evil simply, but a special kind of evil. He concedes, I think, that a world with the evil of suffering in it could be better than one that was entirely painless and pleasant with no logical room for heroism and pity. (Nor would it follow from God's preference for the sterner world that the virtues were the *only* thing he was interested in (p. 207). Happiness and no virtue, virtue and no happiness, might both be poorer than virtue leading to total happiness with some enjoyment on the way.)

But while physical evil is not decisive against God, moral evil, for Mackie, is. If heroism, pity and the rest are the very valuable things that God schools men to, their opposites are the very bad things that God, 'if he were wholly good and omnipotent, would eliminate' (p. 208). The theist says that men's vices are due to their own autonomous wills and that, this allowed, it is self-contradictory to criticize God for making men free and for not making them always do what is right. In Mackie's opinion, a reply along these lines assumes that evils like cruelty 'are logically necessary accompaniments of freedom, just as pain is a logically necessary pre-condition of sympathy' (p. 208). He says that he wants to query this assumption. So do I. We both want to query it for opposite reasons. Mackie, because in the way he almost certainly uses 'free choice' there is no contradiction in 'determined free choice'. I, because in the way the theist has to use 'free choice' (has to, or else he inexcusably calls God both Creator and Judge) there is a contradiction in 'determined free choice'. The point of the theist's reply is that moral evils are not necessary, but necessarily possible, consequences of freedom.

The fact is that Mackie's list (p. 200) of the premisses he needs to make out his case is too short. To the three propositions themselves with their connecting rules of interpretation—rules which amount to this, that 'omnipotent' really means 'omnipotent' and that a good will tries to get rid of evil as far as it can—another premiss has to be added. The other premiss is that the moral

freedom which is the condition of virtue and vice is compatible with determined choices. Mackie makes a gesture towards establishing this when later on he says that there is only one supposition on which God would be blameless for men's actions and that is 'if he made them as they are but did not determine their wrong choices'. That, in turn, is possible only if 'wrong choices are not determined by men as they are' but 'random', 'without value or merit' (p. 209).

Well, if anybody likes to call a choice 'random' because it might have been made differently by the same man in the same circumstances, so that it is not one which he could with concealed manipulation have been made to make, either at the time or at his beginning, I suppose nobody should complain. Nobody should complain unless in the word 'random' there is a request to mean that the man himself did not choose but that the choice happened to him. He chose alright, the man himself, the person who has a character but is not his character, for all the contingency of his choice, and just because of its contingency he is responsible for it and nobody else and its merit or demerit is his and nobody else's.

As Mackie's purpose is to show that the theological position is self-contradictory, and mine is to show that it is not when all the propositions that it commonly includes are brought together, I shall take this view of free will, poorly called 'indeterminist', for granted. If it is not granted, then it is no surprise to theists to be told that the attributes of God are irreconcilable with the moral evil in the world. This is a commonplace in theodicy, though, of course, with famous dissentients. If it is granted and taken as fixing the sense of 'free choice' in the following sentences, then, in their obvious meaning, a self-contradictory omnipotence is asked for.

'If God has made men such that in their free choices they sometimes prefer what is good and sometimes what is evil, why could he not have made men such that they always freely choose the good? If there is no logical impossibility in a man's freely choosing the good on one, or on several occasions, there cannot be a logical impossibility in a man's freely choosing the good on every occasion. God was not, then, faced with a choice between making innocent automata and making beings who, in acting freely, would sometimes go wrong: there was open to him the obviously better possibility of making beings who would act freely but always go right. Clearly, his failure to avail himself of this possibility is inconsistent with his being both omnipotent and wholly good' (p. 209).

There is, by definition, the definition a consistent theism has to give, no logical impossibility in a man's freely choosing the good on every occasion. There is, by definition, a logical impossibility in God's making him freely choose the good on any occasion. 'The

best of all logically possible worlds' (p. 207), may be as impossible as 'the crookedest possible line'. Supposing, though, that such a world could be, still, it could not as such be created if its events included the decisions of free agents with the choice of good and evil. For if there was no difference between two worlds except that in the first right was always done and in the second wrong sometimes, one would be better than the other, yet its superior excellence could not have been the effect of an omnipotent fiat.

Similarly, there is no 'paradox of omnipotence'. 'Can an omnipotent being make things which he cannot subsequently control?' If he cannot he is not omnipotent; if he can he will not be omnipotent (p. 210).

The traditional doctrine is that God in one timeless act creates and providentially orders his creatures through all their times. The doctrine will not allow the question. Mackie will not allow the doctrine and in any case it would look evasive here. However, part of the meaning of omnipotent is 'always able to control everything'. The nature of the control logically depends on the nature of the agent controlled. The control of agents without freedom is direct and compulsive. The control of free agents has two aspects. In consistency, one must be the permission of self-determined actions. The other is the bending of their consequences to fulfil an overall purpose.

With these clarifications, part of the meaning of 'omnipotent' is 'always able to control everything', and that cannot be a true description of God, and 'sometimes unable to control some things' also a true description of him. To have the paradox of omnipotence we should need to be able to ask whether God can create something that he can always control that he cannot always control.

I want to go back to the passage (p. 209 of Mackie's paper), which I hinted might have an unobvious meaning. Parts of it read with a faint ambiguity. Even if 'freely' is given the 'indeterminist' sense, there is no necessary contradiction in saying that there was open to God the possibility of 'making beings who would act freely but always go right'. For it might be meant not that God could have made beings who would freely and necessarily, but beings who would freely and contingently, always go right. God, knowing how every possible free agent would behave in every combination of circumstances, ought to have created only those whom he could not casually predict, but see, as never once turning away from goodness. The trouble is that, for all we know, there might be none, at least under conditions where there would be real merit in what they did. We are quite in the dark about such hypothetical actualizations of possible free choices. And since the purpose of a free choice between good and evil is that there might be real merit in the good choice, perhaps things are better disposed the way God has disposed them than if he had adopted a policy which Mackie in one place recommends, that of leaving men 'free to will rightly'

but of intervening 'when he sees them beginning to will wrongly' (p. 210), the policy that is of promoting sham fights in which moral victories are always won because nobody is ever allowed to lose.

In general, the Butlerian reminder: We just do not know whether the utopian universes which it is supposed a God would have made if he had been good or wise or powerful enough are possible, and if possible, better than the one we have got. And our ignorance of the whole of God's providence is bound to make us incompetent critics of part of it.

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THE PARADOX OF OMNIPOTENCE

In his article on "Evil and Omnipotence" (Mind, vol. LXIV, No. 254, April 1955) Mr. Mackie formulates the question "Can an omnipotent being make things which he cannot subsequently control?" and this he says is a "proper question" because "it would make perfectly good sense to say that a human mechanic has made a machine which he cannot control". Yet, he claims, it leads to paradox.

1. Now it is true that it makes perfectly good sense to assert either or both of the following propositions:

"A human being has made something which he cannot control."

"A human mechanic has made something which he cannot control."

But it does *not* then follow that it makes perfectly good sense to assert either or both of the following propositions:

"An omnipotent being has made something which he cannot control."

"An omnipotent God has made something which he cannot control."

These would only follow as perfectly good sense if we supposed (in the first case) that the word "omnipotent" had the logical placing of "human", and if we also supposed (in the second case) that the word "God" worked like "mechanic". But if we supposed all that, we should, and not for the first time of course, have been deceived by grammatical form. For "omnipotent" has to work differently from words like "human", "skilled", "knowledgeable", if it is to make an appropriately odd logical claim for the word "God"; and ironically enough this is precisely its job: to claim that God isn't a mechanic. So the question which sets the paradox, despite Mr Mackie's assertion, is *not* a proper question, and it is not a proper question precisely because of the notion of omnipotence which is nothing if not logically odd. So the paradox arises, and the question is improper, simply because they give the word 'omnipotent' a wrong logical placing.

2. More constructively my point may be put like this:

We may think of a sequence:	*Which is exemplified by (say):*	*Or:*
scarcely powerful	a constitutional monarch	parish councillor
rather powerful	a mayor	Privy councillor
definitely powerful	a Cabinet Minister	City Councillor
very powerful	a Prime Minister	shopkeeper
most powerful	a Civil Servant, or perhaps a Party Chief	shop steward

and we might then suppose that all-powerful is a last term in such a

series, being reserved, not for monarchs, shopkeepers, councillors, Ministers of State, Party Chairmen, but for God.

But consider now other sequences :

of polygons	*of co-planar points*	*of sums e.g.*
		1 term : 1
regular polygon of five sides	1 point	2 terms : $1 + \frac{1}{2}$
„ „ „ six „	2 points	3 „ : $1 + \frac{1}{2} + \frac{1}{2^2}$
„ „ „ seven „	3 „	4 „ : $1 + \frac{1}{2} + \frac{1}{2^2} + \frac{1}{2^3}$
„ „ „ eight „	4 „	
. . . . .		. . . . .
. . . . .		. . . . .
„ „ „ n „	n „	n „ $1 + \frac{1}{2} + \frac{1}{2^2} + \ldots \frac{1}{2^{n-1}}$
etc. etc.,	etc.	etc. etc.

Such sequences might lead us to talk of an " infinite " regular polygon ; of an " infinity " of points ; of an " infinite " sum, as we talked before of " all " or " infinitely " powerful. But in no cases do these specify last terms of the sequences we have given. Rather are they phrases of a peculiar logic which somehow develop and use the sequences, so as to relate them respectively to " circle ", " curve " or " 2 ". For example, as is done in elementary mathematics, such sequences may be traversed ; such routes followed, and suitable stories told. But the hope is always that the ' penny will drop ', the ' ice will break ', the ' light will dawn '—in more philosophical language that " mathematical insight " or " intuition " will be induced, and then in such a significantly different empirical situation words of another logic altogether will be commended ; words such as those we have mentioned : " circle ", " curve " or " 2 ". In other words, phrases like " infinite polygon ", " infinity of points ", " infinite sum " :

(*a*) show a direction in which polygon, point, or summation stories may be developed if we seek to increase our " mathematical insight ", and

(*b*) then plead, by their logical strain, that compared with " polygon ", " point " or " sum ", words like " circle ", " curve ", and in this case " 2 ", have respectively an odd logical status.

Further, it is the function of mathematical logic and analysis to provide some sort of reasonable bridge between the sequences and the words whose logic is problematically odd ; between " polygon " and " circle ", " point " and " curve ", the given series and " 2 " ;

to do something to display the logic of the move which the qualifier "infinite" effects and represents.

I suggest that the theological case is not all that different. We begin with a model such as "power" or "goodness" or (to take an attribute which Mr. Mackie overlooks in his article) "wisdom". These are logically like 'polygon', 'point', or 'sum'. The next move is to tell a tale about such a model until 'the penny drops', etc. Now it is precisely part of the logical function of a qualifier like "all" to develop such a tale in the right direction. If we hope to induce "theological insight", to evoke a characteristically theological situation—what may be compactly called "worship"—it would be no use for example to trade the sequence down from shop-steward to parish councillor; from Party Chief to Constitutional Monarch. We must be sure to tell the tale in the right direction. When the penny does drop, a qualifier like "all" has then a second logical function which can be related to the "difficulty"—the logical strain—of phrases such as "all-powerful", "omnipotent", "infinitely powerful", and the rest. In short, by the logical effrontery it gives to a phrase, the qualifier bespeaks an odd logical placing for the word—"God"—which the phrase characterises; precisely as "infinite" in "infinite polygon" pleads a relatively odd placing for the word "circle". Just *how* "God" is related to words like "power" on the language map is, as in the mathematical parallel, a matter for theological analysis, and it is a special problem for such theological mapping to show how "God" can be related to "evil". But there is nothing insuperable in such problems, once logical empiricism has given us the tools for the job.

Meanwhile we can set aside as irrelevant any pseudo-questions which seem to block our way and which arise because, *e.g.* "omnipotent" is given the logical placing it could not have if it is to make the claim about God which theists wish to make. Not surprisingly, all kinds of fun and games arise if we suppose "omnipotent" to have for the theist a logical placing he cannot give to it—McTaggart was one of the first to enjoy himself in this way. But such philosophical play has no more to it than the mathematical recreations at parties which depend on taking infinite series as finite series but considerably longer.

3. What Mr. Mackie's treatment shows is that the "paradox of omnipotence" can be avoided if "omnipotent God" is given the logical placing of a "sovereign institution"—which shows how far we have had to go already from the logical placing of "human mechanic" or "human being". But why give up the game there and suppose that because the paradox is avoided we have discovered the exact logical placing of "omnipotent God"? Why suppose that we have found a logical synonym from which we can read off various characteristics, *e.g.* "discontinuity" which must then be transferred to our language about God. Once again, this only

shows that we have not yet succeeded in giving "God" the logical placing the word must have, to satisfy the language which in fact religious people use. Even so, Mr. Mackie's treatment goes far enough to illustrate my claim that the task of the empirical philosopher is to do for theology what mathematical analysis has done (say) for the theory of functions: to show, when problems arise, *e.g.* about evil and omnipotence, just what logical settings (if any) can be given to traditional phrases if they are to continue to be used significantly. But this means that the "paradox of omnipotence" has a rather different point from that which Mr, Mackie's conclusion would give to it, and it is this point I have tried to develop in this note.

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VII.—CRITICAL NOTICE

Fact, Fiction, and Forecast. By NELSON GOODMAN. University of London. 15*s*.

GOODMAN says, at the beginning of the second essay in this book, that the problem of counterfactual conditionals arises for him because entities or experiences which are possible but not actual are unacceptable to him without explanation. 'You may decry some of these scruples and protest that there are more things in heaven and earth than are dreamt of in my philosophy. I am concerned, rather, that there should not be more things in my philosophy than there are in heaven or earth.' Goodman thinks that it is impossible to talk of events that did not happen and to discuss what the effects of these events would have been. Yet this is what counterfactuals appear to do. Therefore, like most philosophers who have considered the problem, he conceives his task to be that of giving an analysis of statements about what would have happened, if something which did not happen had happened, into statements about what did happen and what will happen. He says in the first essay that the problem is 'to define the circumstances under which a given conditional holds while the opposing conditional with the contradictory consequent does not hold'. Now although this is a task it cannot be the problem: for the statement of the problem would make clear the reasons for asking for an analysis, it would tell us why we need to try to find another way of saying what the counterfactual itself already says. Goodman makes no attempt to discover what it is about counterfactual conditionals and possible entities which makes it difficult for him to accept them; he assumes that he is correct in rejecting them at their face value and therefore that the only solution to the problem is to find a definition of them in terms of things which he is able to accept. It is as though Kant had produced the Critique of Pure Reason because of some vaguely felt difficulties about our knowledge of things in themselves rather than because of the arguments of Hume which created these difficulties.

Now it may be that the solution to the problem of counterfactual conditionals lies, not in giving an analysis of the conditionals, but in investigating why the difficulties arise, why an analysis is thought to be necessary, and in showing that the reasons for asking for an analysis are not good reasons. This belief is supported by the implausibility of the analyses which philosophers have offered in the past and of the new and ingenious analyses which Goodman discusses in this book.

According to the first analysis which Goodman proposes a counterfactual is equivalent to a statement of a general law and some of the

initial conditions of that law. For example, ' If that match had been scratched, then it would have lit ' is equivalent, on this analysis, to the general law-like statement ' If any dry match is scratched in oxygen, then it lights ' and the initial conditions ' The match was dry ', ' Oxygen was present '. Together, these statements allow the inference of ' This match lit ' from ' This match was scratched '. It almost seems as though Goodman has found what he set out to find, for he has found a description of actual fact which makes no reference to possibilities or to contrary-to-fact suppositions and yet which justifies the inference of the condition mentioned in the consequent of the conditional under analysis from that mentioned in the antecedent. For the law and initial conditions, in conjunction with the antecedent of the conditional, entail the consequent. But this appearance is an illusion, Goodman has not found what is required, for although the truth of the law and initial conditions entails that if the antecedent of the conditional is true, then the consequent is true, it does not entail that if the antecedent were true, then the consequent would be true : if the antecedent were true, then the law and initial conditions might themselves be false. As Goodman himself says, what actual conditions are relevant to the truth of a contrary-to-fact conditional depends on which of them would have remained the same had the antecedent been true and not false.

Moreover, the law and initial conditions entail far more than the conditional of which they are offered as an analysis. Someone who says ' If that match had been scratched, then it would have lit ' may say this because he believes that the match was dry and that dry matches light when scratched in oxygen, but he may also say it because he believes that the match was wet and that wet matches light when scratched in carbon dioxide : what he says does not tell us. Neither the statement of law nor the statement of initial conditions are entailed by the original conditional. It is always possible to say about the assertion of a counterfactual conditional, ' You are right, but for the wrong reasons '.

Goodman himself finds two difficulties with the analysis. He cannot decide what initial conditions are specified in the counterfactual assertion and he does not know how to distinguish between those general statements to which counterfactuals may plausibly be equivalent and those to which it is obvious that they cannot be equivalent.

Goodman considers the possibility that a counterfactual conditional asserts a complete description of the actual state of the world at the time in question. Then, for example, ' If this match had been scratched, it would have lit ', asserts the law ' If a match is dry, is in oxygen, and is scratched, then it lights ' and the initial conditions ' This match was dry, it was in oxygen, it was not scratched, it did not light, etc.'. But the conjunction of the law and all the initial conditions allows the inference of any statement from the

antecedent (' This match was scratched ') for they contradict it. Therefore if ' If this match had been scratched, it would have lit ' asserts the law and the initial conditions, it is a statement that allows the inference of any consequent from the antecedent. The analysis of ' If this match had been scratched, it would have lit ' is the same as that of ' If this match had been scratched, it would not have lit '. This analysis provides a statement by means of which the consequent can be inferred from the antecedent, but it provides one by means of which any consequent can be inferred from the antecedent. Goodman considers several ingenious escapes from this difficulty but does not find one that he considers satisfactory.

The second difficulty that he finds with the analysis arises from the familiar problem of distinguishing between statements of law and statements of mere generality. Those general statements that are merely descriptions of what is actually the case do not entail counterfactuals, whilst those general statements that do entail counterfactuals are not merely descriptions of what is actually the case. But unless these law-like statements that do entail counterfactuals can be explained as descriptions of actual facts, Goodman's analysis has achieved nothing. For it would have reduced mysterious counterfactuals to mysterious laws; counterfactuals which referred to possible events into laws which discussed possible events. Goodman suggests that the distinction between laws and general descriptions, between, as he puts it, ' causal laws and causal facts ' is not one of meaning but of use. If I am willing to accept a statement on inductive evidence and then to use it to predict other facts, then I am using that statement as a law. If I will not accept a statement until I have examined separately all its consequences, so that I will not use it to make any predictions, then I am using that statement as a statement of causal fact.

One way in which Goodman expresses this distinction is certainly mistaken. He says ' As a first approximation, then, we might say that a law is a true sentence used for making predictions '. But it is quite possible to discover an accidental generality about the future and use it to make predictions. For example, someone might discover that all the people at some meeting will vote against a certain proposal. But the general assertion that he makes and uses for prediction is not a law. We certainly cannot say that a law is a true sentence used for making predictions.

But Goodman's other way of expressing the distinction is not open to this objection. If someone makes an assertion on inductive grounds, then in all probability he asserts a causal connexion. A person who tests a sample of apples from a tree and finds them all good cannot argue to the generalization that all the apples from that tree are good if he also believes that there is no connexion at all between the apples being good and their having grown on that tree. Nevertheless I do not think that this explanation of the

distinction between law-like statements and general statements is the fundamental one. If Goodman is right, then the same statement may be used by one person to assert a law and by another to assert a generality. Both assertions would make the same statement, a statement that merely describes the actual events of the world, but one man would be prepared to accept the statement on inductive evidence and the other would not. But it often happens that a generality is true whilst the corresponding law-like assertion is false. Therefore the two assertions cannot both make the same statement. Goodman's analysis allows the possibility that a true generality may not be a law, because it is possible that no one will accept this generality on inductive evidence and then use it for prediction, it is possible that no one will use that statement as a law ; but it does not allow the possibility of a statement that *is* used as a law being false as a law but true as a generality.

Goodman makes another attempt at the problem of counterfactual conditionals by first analysing dispositional predicates. Then he can equate, for example, ' This is inflammable ' with ' This would burn, if . . . '.

He wishes to define dispositional predicates in terms of predicates which refer only to actual events happening to existing things ; these predicates he calls manifest predicates. (This distinction between manifest and non-manifest predicates is not clear. For example, is ' wooden ' a manifest or a non-manifest predicate ?) He points out that a predicate, such as ' inflammable ', always has a corresponding manifest predicate, in this example it is the predicate ' is burning '. He starts with ' is burning ' as a tentative, though incorrect, definition of ' inflammable ' and goes on to improve this definition. He suggests that a property that is a causal concomitant of burning and is a manifest property may be used to extend the application of the predicate ' inflammable ' to things which are not burning, have never burnt, and will never burn. Many things which at some time burn are made of wood, and this is no mere accident but a causal connexion, so ' inflammable ' might be taken to mean ' has burnt, is burning, will burn, or is made of wood '. And if other properties are discovered which accompany burning but which can be tested when ' made of wood ' cannot be tested, then these can be added to the definition.

Goodman goes on to apply this solution to those contrary-to-fact conditionals which do not discuss merely what would have happened to an object had it been in different conditions, but rather what would have happened had an object existed which did not exist, or an event occurred which did not occur : that is to the analysis of statements about possible objects and possible events. This he does by interpreting all statements about objects which do not exist, but which might have existed, as statements about objects which do exist but which might have had properties they do not have. For example, a statement about an accident which a train

did not have in passing over a bridge would be interpreted as a statement about the train passing over the bridge. 'An accident might have occurred in passing over the bridge' becomes 'The passing of the train over the bridge was accidentable'. The analogy with 'inflammable' is apparent. All talk of possible objects is construed as talk about the dispositional properties of existing objects. Sometimes Goodman is led rather far in his search for suitable existing objects. In one example he considers the object consisting of the body of one motor car and the chassis of another.

This analysis has the consequence that no one can understand a dispositional predicate before he knows of a property which is causally connected with the corresponding manifest property. But someone might understand 'inflammable' without knowing any tests for inflammability except that of attempting to set light to the object.

Both of Goodman's attempts to reduce statements about possibilities to statements about actualities require that a causal law should differ from a statement of accidental connexion but yet do no more than describe the actual events and conditions of the world. As I have already said, he attempts to show that a causal hypothesis is one which we use to make predictions and which we regard as confirmed when we have verified some of its consequences. This explanation in its turn requires an explanation of why we regard some, but not all, hypotheses as confirmed by their instances. Goodman gives examples of this kind: if we examine nineteen of the twenty balls in a bag and find them all red we may regard this as confirming 'All the balls in the bag are red'; we shall not regard it as confirming 'All the balls in the bag are red except the one we have not yet examined and that is black'. So Goodman is led to discuss the problem of induction.

In his discussion of the problem Goodman confuses two questions. He confuses the question whether our predictions about the future can be justified with the question whether inductive arguments, that is arguments from particular cases to general laws, can be justified. Now it may be that we can justify our predictions about the future, but that this justification makes no use of the inductive argument and that the inductive argument cannot be justified. He fails, too, to make a distinction which I believe is fundamental here, though most philosophers are of his opinion rather than mine, between the question whether our predictions can be justified and the question whether we know them to be true.

He is right in arguing that the inductive argument, the argument from a number of instances of an hypothesis to the truth of that hypothesis, cannot be justified either on logical or factual grounds and that it is neither logically nor factually valid. He is right, too, in pointing out that the argument is inconsistent, since it can be used to argue from the same premises to two contradictory conclusions. He shows this by means of an example similar to the above

example about the red balls in a bag. Since the argument is inconsistent it follows that it is not valid even to the conclusion that an hypothesis is probable, for it can be used to give equal probability to a prediction and its negation. The same holds, as Goodman shows, for the conclusion that an hypothesis is confirmed. Consequently he is correct in holding that the old problem of induction has been shown to be insoluble : the inductive argument cannot be justified because it is invalid and, what is worse, inconsistent.

He is wrong in arguing that none of our predictions are valid and that we cannot know them to be valid. He says, ' If the problem (of induction) is to explain how we know that certain predictions will turn out to be correct, the sufficient answer is that we do not know any such thing '. But why should anyone read books or conduct researches if he did not believe that he would come to know things that he did not know before?

Now Goodman is on the horns of a dilemma. On the one hand he has proved the inductive argument to be invalid and on the other he believes that only by means of the inductive argument can we justify our belief in laws or in predictions. He goes between the horns. He invents a meaning for the term ' valid argument ' in which to say that an argument is valid is not to say that it never leads from true premises to false conclusions. Now it may be that a meaning of this kind can be found in which the inductive argument, or some restricted form of it, is ' valid '. But in solving the problem of how an inductive argument can be valid an equally difficult problem is created : why should anyone care whether, in this new sense, any argument is valid or not? It would be reasonable to conclude, since it has been shown that the inductive argument is not valid even to the conclusion that certain evidence confirms a certain hypothesis, that assertions that some hypothesis is confirmed are of the nature of persuasions or directions ; for example, that ' This evidence confirms this conclusion ' means ' This evidence ought to make you accept this conclusion '. Instead Goodman takes the bizarre course of attempting to find a sense of ' valid ' in which the inductive argument *is* valid.

He argues that, when Hume said that it was habit that leads us to make the predictions we make, Hume was proposing that the fact that these predictions were made by habit justified the predictions. Hume, according to Goodman, proposed ' made from habit ' as an analysis of ' valid ' and so put forward a solution to his own problem of induction which Goodman believes substantially correct. Goodman supports this notion of validity by a false analogy with deductive inference. He argues that the validity of a deductive inference consists in its being in accordance with established usage and hence he concludes that the validity of an inductive inference lies in this same fact. Now whether a deductive inference from one *sentence* to another is valid does depend upon how the sentences are used, because whether the inference does not lead from a truth to a falsehood

depends upon how the sentences are used. But whether an inductive inference is valid does not depend merely upon how sentences are used, because whether the inference does not lead from a truth to a falsehood does not depend merely upon how sentences are used. To accept that the validity of an inductive inference does depend upon usage involves either abandoning the view that a valid inference is one that never leads from truth to falsehood or accepting that all truths are truths of logic. Goodman comes near to doing both of these. He says: 'Indeed, I should be inclined not merely to agree that there are no necessary connexions of matters of fact, but to ask whether there are any necessary connexions at all. . .'.

Goodman, then, makes the following distinction between valid and invalid inductive arguments: the valid arguments are those that have an established history, the invalid arguments are those that have never been used before. 'Thus the line between valid and invalid predictions (or inductions or projections) is drawn upon the basis of how the world is and has been described and anticipated in words.' Since it seems to me that this distinction cannot solve any problems about how it is that evidence supports hypotheses or how it is possible to know the future I shall not follow Goodman's development of this idea.

It follows from this view about validity that a statement is confirmable by a valid inductive argument if it is of a kind habitually accepted upon inductive evidence and that a statement is not confirmable by an inductive argument if it is of a kind which people are not generally inclined to accept upon inductive evidence. This same distinction, Goodman believes, is the distinction between statements of causal connexion and statements of accidental connexion. A law is a statement of a kind that is generally accepted upon the basis of some of its consequences. A mere generality is a statement of a kind which is never accepted in this way.

These, then, are the explanations to which Goodman is led in his attempt to reduce possibility and necessity to actuality and contingency. This attempt is forced upon him by his conviction that reduction is the only method of removing a philosophical difficulty.

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VIII.—NEW BOOKS

Toward a General Theory of Human Judgment. By JUSTUS BUCHLER. New York: Columbia University Press, 1951. Pp. 176. $2.75.

THIS is an important book. It would be unfortunate if parochial notions as to what philosophical analysis consists in, or what sort of language it should be couched in, restrict either the number or the seriousness of potential readers. It should interest, in particular, those who have felt Ryle's arguments for rejecting a mentalistic interpretation of experience and, hence, of judgment. For if a well-worn theory has broken down, leaving unfortunate influences in our language, it is not enough to undertake a perpetual hunt for ghosts—someone is going to wonder what sort of general theory can be devised to replace the haunted one. Buchler's orientation in philosophy is in the tradition of Peirce, Royce, Dewey, and Mead rather than Russell, Moore, Wittgenstein, or Ryle and his hopes and fears for philosophical analysis are accordingly different. If some statements in this present book sound strangely metaphysical to the British ear that is because this is metaphysics—but with a difference. Buchler himself describes his theory as a "metaphysics of utterance"—but by that he means a categorial scheme which will help us to illuminate the generic traits of human thinking and doing, especially as this is involved with language, communication, meaning, and judgment. Metaphysics in this sense is an attempt systematically and suggestively to describe the broadest features of the natural world—in contradistinction to the various other worlds sometimes thought to be assumed by metaphysicians. Clearly Buchler is not, however, trying to write either a clumsy kind of poetry or an odd kind of science, even though he demands that philosophical theory give rise to "philosophic satisfaction" (the sort of feeling, I take it, that we have when puzzlement leaves us and we say, "Yes, that is so—and better said than I could have said it") and to verification.

To describe this book as being written in the tradition of contemporary naturalism and empiricism is misleading unless we realize that it is also an attempt to make naturalism and empiricism somewhat different from, and more adequate than, they were before. To describe it as being written under the influence of Dewey would be doubly misleading if (i) we forget that Dewey's philosophy was primarily a new interpretation of experience rather than a new theory of method, and (ii) neglect the fact that it is Dewey's direction, rather than the letter of his writing, with which the present work is in agreement. Still in all there is the problem—is it, in any sense, true? In outlining some of Buchler's points, in terms of which we would judge that question, it is only fair to remark that the thought in this book is very compressed, and it is, avowedly, "Towards" a theory rather than being in fact the completed thing.

In his analysis of the foundations of judgment Buchler introduces a new categorial term "proception" and reformulates, in the light of his analysis of what this term stands for, five other categorial terms: "communication", "compulsion", "convention", "perspective", and "validation". Buchler finds the traditional use of the term "experience" too mentalistic, and the term "behaviour" too physiological or psychological, to be philosophically satisfactory. He wants a term that can refer to experience *and* behaviour—since "judgments", in the most broad sense, are

not *just* "inside me" nor are they *just* "exhibitive". Judgments are "products" of the human being, and the sort of product that they are, can either be acted out dramatically or for the moment be "kept to oneself"—casually modifying one's thoughts and action. The term chosen to indicate the process Buchler is most interested in pointing to is "proception", and it generalizes, supplements, and in some cases replaces, "experience" and "behaviour". Buchler introduces it in the following fashion, "The interplay of the human individual's activities and dimensions, their unitary direction, constitutes a process which I shall call *proception*" (p. 4). The developing proceptive direction indicates the developing individual and his activities. In so saying Buchler is trying to lay down the basis for a theory of individuality which would neither be empirically atomistic, nor which would socialize away, in a transcendentalism, all that is unitary and succinct in the individual. Now proception is so broad a concept that one must distinguish that which is simply relevant to it from that which is generically involved in it. Eating is relevant to proception but it does not generally characterize the proceptive direction (p. 5) even though, I suspect, the early eating experiences of childhood in terms of which I form not only my eating habits but also, in part, my attitude towards my parents and towards my world might characterize that direction pretty thoroughly. But the generic traits that Buchler is most interested in are broader than specific kinds of concrete experiences involved in an individual proceptive direction—they are, roughly, the other five categories which characterize that so broad sense of judgment. To indicate how Buchler stresses the *whole* sense of being a human animal in his use of the term "proception" I quote, "It is the proceiver, then, not a physiological or intellectual capacity of the proceiver, that wonders, asserts, interrogates. These are proceptive functions" (p. 5). However, proception is not *so* broad a term that it includes everything that is ontologically related to the individual; a fly in the Amazon jungle is not a procept but "a fly observed is a procept, for it reinforces the habits of expectation and their limits" (p. 7).

Buchler distinguishes (though he does not separate) three perspectives in, what he calls, the proceptive domain: the *gross proceptive domain*, comprising "all that belongs to the individual's living makeup, the segment of nature within which he functions, the past that is actually or potentially alive for him, the sum of his suppositions, guiding principles, commitments, and peculiarities . . . the class of all his interrelated procepts . . . The *floating* proceptive domain represents the summed-up self or proceiver within a given situation . . . the *immanent* proceptive domain comprises all that is present to—that is, available for—the proceiver at a given moment" (pp. 8 and 9). Two points should be noted here. The first is that Buchler uses the term "situation" to specify what seems to be a technical notion in his scheme; it not only includes what is problematic in the given, but also "the concatenated set of interests, occupations, or problems" that could be declared by the individual. This is a notion often used by Buchler and one wishes that he had devoted more space to its explicit elaboration, though, to be sure, he might reply that the entire book is, in effect, an implicit elaboration of both the notion of "situation" and of the notion of "proception" and its supplementary conceptual apparatus developed in the first chapter. This is so; but elaboration is not always clarification and the harsh critic might suggest that the one is here given at the expense of the other. The second point worth noting is to Buchler's credit and reflects the subtlety of much of

his thought; the immanent domain is neither "essence intuited", nor "an image of self" that a man carries about in his head, "it is the directly available upshot of the whole of the individual's proception" and Buchler hopes he has so defined this as to make senseless any question as to whether one procept is more "present" than another, or whether one or another domain peculiarly "possesses" a certain procept to the exclusion of others. All procepts (all existential complexes relevant to an individual as individual) are *equally* "given", and "unconscious" motives as they partake of and influence the general "direction" of the individual are emphatically *there* in the *proceptive direction*. In this sense "available" does not just mean available to consciousness but, I take it, as an item in terms of which behaviour is influenced. The way I hate or love my mother would figure in all three domains but you might describe that relation differently in each as it would not (necessarily) call attention to itself in the same way in each. What is a procept in one domain is a procept in all since it *is* a procept—but different patterns call attention to themselves in different inquiries. The three divisions of gross, floating, and immanent proceptive domains are not "essential", even though matter of factly the case—but are rather contextual or situational.

Buchler treats "assimilation" and "manipulation" as basic ways of reacting to, and interacting with, the world. They are called "dimensions" of proception and indicate the passivity, on the one hand, and activity, on the other, of this reaction and interaction. These dimensions involve the dynamics of individual action in communicating, inquiring, being convinced, etc., and are involved in the categories which name the other chapters of the book. It is here, perhaps, that one would tend to camp Buchler with Dewey but keeping in mind his explicit (p. 12) rejection of the "epistemological morass" of "Dewey's interaction-transaction" sort of distinction. Still it remains that the natural tendency is, in a review, to go to Dewey's terms, such as they are, to explain Buchler's terms, such as they are. But Buchler would attempt to choose more descriptive, rather than "instrumental", language. Not only is the problematic situation only one kind of situation but also there is an "ineradicable randomness" in proception, "a residuum of irrelevancy . . . aimless passivity . . . (but though) Sheer drift is ubiquitous and undramatic, . . . it often translates itself into one or another form of sensibility" (p. 22).

Experience, then, would be the class of procepts; it is the "directed, cumulative interplay of assimilation and manipulation" (p. 26)—though we can assume that Buchler would rather rid philosophy of the term "experience" for technical purposes, retaining only its conversational use. The qualification "directed", however, is ambiguous, as Buchler himself notes. Surely experience "adds up" to something even as proception does—and even pre-Buchler usage would agree that not only can we usually make out a direction for cumulative experience but also to a great extent we "direct" our experience (and have it directed for us). Yet in casual usage we call even undirected assimilation and manipulation "experience". What exactly does Buchler mean? Is it simply that experience is direction-finding? That would be general information about human life and its activities. Or is he specifying, by convention, that part of loose philosophical usage of the term "experience" that he chooses to retain to indicate its relation to "proception" for his own technical equipment? *i.e.* that which is directed in "experience". But that is not too helpful since he does not retain the notion for technical purposes nor

does he, by virtue of the specification "directed", clarify his usage with the usage of other philosophers. However, as it turns out, Buchler does not intrude the notion of "experience" for "experience directed" since he stays with the concept of "proception" and develops "assimilation and manipulation" in terms of the categories of "convention", "compulsion", etc., to include all the direction-finding that one would want. I suspect that Buchler would argue that any difficulty here arises from the ambiguity of "experience"—leading philosophers to argue whether such-and-such is or is not "an experience", or "part of experience"—he hopes, by virtue of scope and definition, to obviate the possibility of fruitless discussion as to what is or what is not a "procept" or part of the proceptive domain.

Some remarks that Buchler makes concerning the problem of "privileged access" are worth noticing. He would agree with Ryle to this extent: "public" and "private" experience do not designate intrinsically discrete sorts of experiences (or procepts) in two different kinds of worlds. To say that some experiences are "private" is simply to say that they are neither "co-operatively described nor conventionally classified" (p. 27). Most so-called "private" experience would not fit this restriction, or would fit it only more or less; even a "toothache" is a "natural complex . . . a procept (for certain individuals) designated by a common name". We can communicate about toothaches even as we can communicate about the rainfall. In either case we are usually compelled to notice this part of the proceptive domain—and we have the conventions of symbolization on hand to call attention to what we notice. We can retain the "private-public" distinction only in degree and somewhat loosely. To go further would be to say that there are procepts that have "no conceivable or describable basis or source or occurrence" and, minimally, this is a position that one could certainly not "argue" or refer to data to prove.

We have dealt upon Buchler's first chapter because it sets the tone, and defines the equipment, for what is different in the rest of his discussion. It would be only fair, however, to stress even more the other chapters if space allowed since they not only amplify the equipment and refine the tone but really constitute the body of what he wants to "say". In his chapter on communication Buchler makes use of the proposal in his preface—that any issuance of human activity is a product and every product, being a product of a sign-using proceiver, can in some sense be characterized as a judgment. This book could be described as an investigation of the essential conditions underlying human production (*i.e.* making, saying, acting). Communication, which invokes signs by its very nature, is not just an activity that we indulge in now and then; it is a continuous process involved in proception itself not just because we "read nature" off in signs, but also because we are perpetually involved in community (of one sort or another). Buchler does not take this fact as denying uniqueness, but only as denying atomicity, to the individual. And communication, whether with our "selves" or with others, as a deliberate act or not, presupposes the community at the same time that it indicates that the community is not a transcendent "harmonious whole". It also presupposes that an individual proceptive domain in some sense stands apart—precisely due to the multitude of possible procepts and the multitude of actual communities and communal interests.

This all results in de-intellectualizing the concept of judgment. What can be described as a product in one sense (*any* issue of our proception, be

it activity or utterance) can be described as a judgment also, since it involves selection, discrimination, and combination. There is no "faculty" of judgment but rather a functional rendering of procepts. Buchler distinguishes between "assertive" judgments (true or false), "exhibitive" judgments (shaping of materials, including signs), and "active" judgments (acting out). The point is that "exhibitive" and "active" judgments do not presuppose an intellectually stylized "assertive" judgment; and this is so (Buchler would argue) not simply because "assertive" judgments are products too, but because the three distinctions are not "structural" but are functional in the sense that even an "assertive" judgment can be "exhibitive" or "active" (and so on). What gives judgments its "kinds" is what gives selection, discrimination, and combination its "kinds". That in different cases one is both descriptively and contextually more adequate, neither renders the other two classifications irrelevant nor renders certain products "not judgments". There is not a "mental" judge who adjudicates action (Buchler assumes, rather than feels called upon to argue, the breakdown of the epistemology and metaphysics that would underlie such a position).

In the last half of the book Buchler exploits and develops his earlier analysis as well as displays its relation to the more familiar concepts of compulsion (the recalcitrancy of facts), convention (latitude in judgment), perspective (the selective aspect of proception), and validation (justifying appraisal, securing judgment). But to Buchler these terms are not just cognitive concepts—they are generalized in terms of proception. He does not seek to throw light on the formal procedures of science or philosophy but rather to throw light on the nature of man who "queries": who shapes cognitive and expressive enterprises like science, philosophy, art, and religion. There is much more on the relation between exhibitive, active, and assertive judgments but there is also quite a bit on the nature of philosophy, art, and science. Philosophy itself, holds Buchler, is not just assertive like science, or exhibitive like art (nor, indeed, are science and art *just* so). The structure of interrelated judgments which comprise a philosophy is exhibited not asserted. There is a sense in which the compulsiveness of a philosophical perspective (say, this book) indicates its persuasive character but this is generalized beyond the nature of mere emotional appeal. The validation of a philosophical perspective, like validation in general, goes beyond sheer verification; "Philosophic language and philosophic meaning only make greater demands on the inventiveness with which we pursue the process of validation" (p. 139). An idiosyncratic appeal is not enough, for the philosopher claims more, and the requirements of judgment are in proportion to the claim. If one were to chide Buchler with this I suspect he would accept the challenge, for criticism (though not polemics) he holds to be a dimension of philosophy as well as of judgment.

What about all this? Many points could be criticized. There is much that is obscure. Some of the language is barbarous, some of it is too oracular, some of it is seductively pleasing. It is metaphysics. Generalization follows upon generalization. Scarcely a sentence is "logically analysed". No appeal is made to "usage". It would seem to this reviewer that before the argument of this book be accepted or rejected in its own terms (and that would really depend on what Buchler does with what he has) one should accept the prior challenge that lies in the *fact* of the book rather than its content. What should philosophical inquiry entail? Is there a place for constructive systems? Is there a

place for generalization of this type? Is there such a thing as the search for "generic traits" in Buchler's sense? Can one experiment with language—and seek for solutions and illumination in its categorical and analogical nature? Is ready-made language the subject, as well as the tool of philosophy, in a privileged sense? Buchler, certainly, would argue that ready-made language was once *not* ready-made. He would claim his own procedure to be analysis of a more basic kind that is just not in current favour. Clearly, if Buchler is *wrong* in his theory, if it is not adequate, will not stand analysis of the current type, contains errors of fact or of discrimination, it is still and all a different kind of wrongness than contained in a wrong analysis of the "ordinary use of the word 'that'"—and this is because it pretends to a different kind of being *right*. This would be part of the "exhibitive" character of Buchler's book. In this reviewer's opinion it is only one of many challenges to be found there.

DONALD G. BROWN.

The Theory of Speech and Language. By SIR ALAN GARDINER. Second Edition with Postscript. Oxford University Press, 1951. Pp. 348. 15s.

THIS book, which discusses Speech and Language from a grammarian's point of view, contains a fair amount of philosophically interesting material, although philosophers may find the points which are really illuminating for their purposes more scattered than they like.

Gardiner uses the term "Speech" to cover all utterances, written or spoken, as opposed to the material of Language out of which we run up these *ad hoc* constructions. Speech, he asserts, involves the interaction of four factors, *viz.* (1) Speaker, (2) Listener, (3) Words, and (4) Thing meant. The analysis of meaning which follows is not satisfactory. He tries to distinguish the thing meant by a word from its meaning but his explanation of this distinction is inconsistent: it seeks unsuccessfully to make plain how such different words as "Pussy", "centaur", "is", "very", etc., correspond in the same sense to "things meant". He seems to accept uncritically the view that every legitimate word corresponds to an object and a not too explicit belief in propositions as mental entities further confuses the exposition: "things must occur to our minds before they can be clothed in words" (p. 32). This is not to say that his underlying contention is pointless. In his insistence that a study of Language should attend to the speaker's intention and the listener's expectations in particular situations, he allies himself with those who might be most inclined to quarrel with some of his terminology. Later on, he defines "the thing meant by any utterance" more simply as "whatever the speaker has intended to be understood from it by the listener" (p. 82), and the meaning of a word as "the multitude of ways in which a speaker may if he will legitimately employ it" (p. 100).

He analyses next a particular act of speech—the exclamation "Rain!" addressed by James to Mary with whom he is about to take a walk. The two most important points which emerge are the surprising range of what one exclamatory noun can be intended to convey ("Just look!", "It's started", "We can't go", "Bother!" and so forth) and the vital contribution which the situation or setting makes to this range. He concludes that the criteria commonly accepted as entitling an utterance to the rank of sentence need re-examining along with the standard classification of sentences into Statements, Exclamations, Questions, and Commands or Requests.

This is followed by a discussion of the Sentence. Gardiner maintains unhesitatingly that "Rain!" etc., uttered in the right circumstances, ought to be called sentences. This rules out any formal definition of a sentence. The test he recommends is whether an utterance, together with the clues provided by its situation, discloses a communicative purpose. "A sentence is a word or set of words revealing an intelligible purpose" (p. 98).

Then, after describing the characteristics of word order and intonation by which we identify assertions, commands, etc., and which act as clues or signals as to how a sentence should be taken, he turns to the interesting matter of "incongruent form". Just as the grammatical family associations of a word can clash with its context (*e.g.* "the boy king", etc.), so these clues often run counter to the real function of the sentence, *e.g.* "Are you going to shut that door?" His provisional conclusion about these cases is that, whilst the fourfold classification of sentences is valid, any one type of sentence possesses, at least in rudimentary form, all the characteristics of the other types. The fourfold classification only refers to the quality which pre-dominates over the others. Examples of incongruent form are just the sort of hybrids we ought to expect.

He rejects the claim that a sentence must be analysable into subject and predicate. The saving qualifications, such as the admission of a subject present in thought, which are added to cover such respectable sentences as Imperatives with no second person inflexion, evade the plain fact that these words are used without a subject. This test also fails to distinguish sentences from subordinate clauses. Nevertheless he holds that the subject-predicate nexus is somehow implicit in the sentence, since to be informative involves saying something noteworthy (a predicate) about something specified or indicated (the subject). This is plausible in the case of simple exclamatory predicates, where the situation, or even pointing, supplies the needed indication. How it applies to exclamations like "Hullo!", which presumably also often pass the test of "revealing an intelligible purpose", is not clear. He suggests that the exclamatory predicate was the natural form for primitive speech to take, the subject being either omitted or perhaps slipped in by way of an afterthought as a concession to the listener's slow-wittedness. Such forms as "Wonderful, that concert!" may hark back to the primitive word-order.

Finally, he illustrates and justifies his earlier assertion that the four accepted classes of sentence overlap, showing how question-form can be used to make an assertion, an exclamation, or a command, and so forth. This is a matter which undoubtedly calls for more thorough investigation than it has received. We are all too easily tempted to adopt such vague logical map-references as "prescriptive use of language" without realising how unspecific they are. Unfortunately Gardiner's account is too sketchy to fulfil this need as it stands.

Throughout this book there is a refreshingly common-sense approach to the jargon of grammar. It also makes a number of philosophically helpful suggestions. Philosophers will be encouraged to seek them out by Gardiner's lucid and attractive style.

J. H. SCOBELL ARMSTRONG.

On Philosophical Style. By BRAND BLANSHARD. Manchester University Press, 1954. Pp. 70. 5s.

PROFESSOR BLANSHARD has written a pleasant essay of about 13,000 words in favour of style and clarity in philosophy. It is preaching to the

well behaved, of course. The ill behaved will not listen. But the well behaved will enjoy Professor Blanchard's urbanity, his quotations, and his wit; and perhaps some beginning writer, who has not previously considered the matter, may from reading this essay conceive the root desire, the desire to be polite to his readers and therefore to be clear.

RICHARD ROBINSON.

New Studies in the Philosophy of Descartes. By NORMAN KEMP SMITH. Macmillan, London, 1952. Pp. xii + 369. 25s.

Descartes' Philosophical Writings. By NORMAN KEMP SMITH. Macmillan, London, 1952. Pp. viii + 316. 25s.

PROFESSOR KEMP SMITH'S *New Studies* is in no sense a revision of his *Studies in the Cartesian Philosophy* of 1902, but is a re-interpretation of the Cartesian system made in the light of work on Descartes done in the intervening period, especially that of Gilson.

Professor Kemp Smith does not directly invite us to consider how far Descartes' views are relevant or helpful to us in considering modern philosophical problems. His work is historical and exegetic and his main concern is to show "how well aware (Descartes) became of the difficulties to be overcome . . . and how he dissociated himself, ever more definitely, from several of the positions to which he had first inclined" (p. v).

Professor Kemp Smith's interpretations are persuasive and well-documented. The documentation provided by copious notes and quotations is further supplemented by the selections translated in *Descartes' Philosophical Writings*, which should, if possible, be used with the *New Studies*, as the translation often helps to make the interpretation clear.

I propose to discuss only three points among the many raised. First the account of the development of Descartes' doctrine of innate ideas. Professor Kemp Smith shows that in the *Regulae* Descartes held no theory of innate ideas and no theory of representative perception in the sense that physical realities can be known only by way of mental duplicates (p. 51). The simple natures are *ingenitae* only as being the constant properties of the body with which the mind is conjoined. Awareness is purely passive and is construed always on the analogy of sight. This interpretation is made clear by the translation of *conceptus* in the famous definition of *intuitus* (Rule III) by "apprehension" and not by "conception" as in Haldane and Ross. The immediate objects of apprehension are patterns in the brain. This early view of Descartes, which seems to be implicit also in the curious passage from the *Dioptric* usefully included in the *Philosophical Writings*, seems to be queerer than Professor Kemp Smith perhaps recognises and it is rather misleading to describe it as "empirical-realist". A better designation is the paradoxical "physiological idealism". However this may be, Professor Kemp Smith argues convincingly that by the time of the *Meditations* Descartes had arrived at a theory idealist in a non-paradoxical sense, *i.e.* that innate ideas "not to be reckoned corporeal" enter into all *judgment* as distinct from apprehension. Here again he shows, without commenting on, the extreme oddness of Descartes' doctrine, particularly of the view that innate ideas are "innumerable". On this point we should have been grateful for a little more detached comment. The modern philosopher reflecting on the distinction between

empirical and *a priori* finds the theory of innate ideas curiously remote. But it is an attempt to clarify this distinction. We find ourselves in the position of understanding in a sense why Descartes says what he does, but not understanding at all what it is that he is saying. Given his terminology and his premisses, we see that these are the things that must be said, yet what is it that they mean and how are they supposed to explain our *a priori* knowledge ?

The second point I should like to discuss is the argument in Chapter III that Descartes *from the beginning* was aware that the purely deductive method in physics must be supplemented by experiment and observation. Apart from Universal Physics, which reveals only the many possibilities, the science of nature must proceed by a hypothetico-deductive method, the hypotheses being suggested by observation and tested by experiment. This interpretation is well substantiated not only by Descartes' theoretical account, but also by his actual practice in Rule XII. Yet those who complain of Descartes' excessive *a priorism* are still in the right. It is not so much that he misconceived the whole method as that he took for self-evident truth what is in fact hypothesis, *e.g.* the possibility of the mechanical explanation of all physical change is a hypothesis. Descartes took it for a necessary truth, which put a limit to all possible hypothesis.

Thirdly, Professor Kemp Smith holds that Descartes was forced by the sheer difficulties of his position to abandon the "angelic, purely spiritual view of the self" in which he was "entrapped" (p. 159). This point is made with special reference to the letters to Regius and the Princess Elizabeth which are translated in the *Philosophical Writings*. Professor Kemp Smith follows Gilson in suggesting that there is here a withdrawal from the original "two substances" view of mind and body put forward in the *Meditations*. I am not convinced that Descartes was ever prepared to retract this view. What he says to Regius and to Elizabeth is for different reasons doubtful evidence. The rash use by Regius of the term *ens per accidens* had exposed Descartes himself to the charge of heterodoxy and we know that he was prepared to hedge in order to avoid this charge ; and it will always be doubtful how much real respect for Elizabeth's intellect is concealed behind Descartes' courtly deference. In the letter to Arnauld (also translated) he seems to take back what is granted in the letter to Elizabeth, *i.e.* that the soul is in the ordinary sense corporeal. It also seems clear from the replies to the Sixth Objections that Descartes, however hard-pressed and however willing to make verbal concessions, was unwilling to give up his conception of the mind as a substance. It could be plausibly argued as by Clauberg and by many others before and since that this conception is the only one which gives sense to the doctrine of human immortality to which Descartes was committed by his religion. It would not be surprising therefore if Descartes refused to abandon it, although the passages show, he certainly became increasingly aware of its difficulties.

I have failed to touch on many points which Professor Kemp Smith has treated in a most interesting manner. Special mention should be made of Chapter VIII which deals with the relation between Descartes' Physics and his General Philosophy.

The companion volume of translations has been mentioned in passing. It is clear and readable and comprises the *Discourse* and *Meditations* and extracts from *The Passions of the Soul* and *The Search after Truth* besides the works and passages mentioned above.

M. KNEALE.

Logik und Existenz. By ALBERT MENNE. Meisenheim/Glan : Westkulturverlag Anton Hain, 1954. Pp. 153. Price not indicated.

THE main topic of this clearly written and carefully argued book is the apparent divergence between the classical theory of the categorical syllogism and the modern systems of logic of the Frege-Russell type. While it is comparatively easy to achieve a natural logistic transcription of the classical laws, it is by no means obvious how, if at all, one can so transcribe the four types of categorical judgment—*A*, *I*, *E*, *O*—while preserving the validity of the classical laws. The transcriptions *e.g.* of '*SiP*' into '$(\exists x)(S(x)$ & $P^*(x))$' and of '*SaP*' into either '$(x)S(x) \supset P(x)$' or into '$(x)(S(x) \supset P^*(x) . \& . (\exists x)S(x))$' are easily seen to be inadequate as they do not preserve the laws which are represented by the square of opposition.

The author examines and rejects with good reason these and similar attempts at incorporating the classical theory into the predicate-calculus. After discussing various approximate representations of the theory in the algebra of classes by Boole, Venn and Schröder, he considers the attempts by Bochenski, Lukasiewicz and others to formulate it by means of the propositional calculus together with new functors defined by new axioms. However, even in these cases, the interpretation of the enlarged calculus is not free from difficulties.

Mr. Menne then shows that the classical theory can be "incorporated into" the logic of classes if we consider *all* the possible relations between "definite" classes, *i.e.* classes which are not empty and have non-empty complements. There are seven mutually exclusive and jointly exhaustive relations between definite classes: K_1 — bilateral inclusion between S and P; K_2 — S is unilaterally included in P; K_3 — P is unilaterally included in S; K_4 — S overlaps with P *and* the intersection of $\overline{S}$ and $\overline{P}$ is *not* empty; K_5 — S overlaps with P *and* the intersection of $\overline{S}$ and $\overline{P}$ is empty; K_6 — S and P have no element in common *and* the intersection of $\overline{S}$ and $\overline{P}$ is not empty; K_7 — S and P have no element in common *and* the intersection of $\overline{S}$ and $\overline{P}$ is empty. (In the cases K_1, K_2 and K_3 the intersection of $\overline{S}$ and $\overline{P}$ cannot be empty since both terms are assumed to be definite classes). The two distinct cases of overlap—K_4 and K_5—and of exclusion—K_6 and K_7—are not usually distinguished, although Keynes (*Formal Logic*, 1894) had made the distinction and used it for the definition of the four classical types of categorical judgment: '*SaP*' being defined as 'K_1 or K_2'; '*SiP*' as 'K_1 or K_2 or K_3 or K_4 or K_5'; '*SeP*' as 'K_6 or K_7'; '*SoP*' as 'K_3 or K_4 or K_5 or K_6 or K_7'.

Mr. Menne has rediscovered the Keynesian sevenfold "classification" and the possibilitiy of defining the *A*, *I*, *E*, *O* judgments in terms of it and shows that these definitions allow us to incorporate the classical theory into class-algebra without the use of special functors. He adds an interesting account of various related topics which arise from this situation. Among other things he discusses the possible deductions from two premisses in cases where the premisses and the conclusion are statements of simple relations between definite classes and not, as in the classical theory, of alternations of such relations. He also examines those class-relations for which the requirement of definiteness (non-emptiness of classes and their complements) is dropped. This discussion leads in turn to an examination of logical existence and the null-class.

'Existence' is regarded as a many-levelled (mehrschichtig) conception with 'logical existence' as its lowest and 'real existence' as its highest

level—other types of 'existence', *e.g.* the 'poetical existence' of centaurs lying between these extremes. Logical existence is conceived as internal consistency. This part of the book stands, it seems to me, in need of some elaboration without which, for example, the statement that freedom from contradiction "corresponds to the existence-concept of *Principia Mathematica*" (p. 97) is unconvincing.

Mr. Menne's monograph has, I believe, advanced our understanding of the relation between classical and modern logic. It provides clear answers to clearly formulated questions and suggests further questions and fruitful lines of inquiry. Frequent references to the history of logic, especially in the last century, add to its interest.

S. KÖRNER.

The Neurophysiological Basis of Mind. The Principles of Neurophysiology. By JOHN CAREW ECCLES. (Being the Waynflete Lectures delivered in the College of St. Mary Magdalen, Oxford, in Hilary Term, 1952.) Oxford: The Clarendon Press, 1953. Pp. xi + 314.

THIS book is based upon the Waynflete Lectures given by Professor Eccles in Oxford in 1952. Unfortunately, the title is misleading. No attempt is made to provide an explanation of mind, neurophysiological or otherwise, and the somewhat perfunctory discussion of the body-mind relation in the final chapter contributes nothing new to this time-honoured conundrum.

The scope and content of the book are very adequately indicated by its sub-title. It is, in fact, an admirably clear and detailed account of modern neurophysiology, special attention being paid to the synapse—the junction between one neurone and another. It soon becomes clear that recent advances in technique—to which Professor Eccles has himself made brilliant contributions—have radically altered our ideas about synaptic transmission. In particular, it is becoming increasingly difficult to sustain the older theories of excitation and inhibition in the central nervous system which were exclusively based on electrophysiological considerations. Professor Eccles suggests that these processes may best be interpreted in terms of chemical changes at the cell-membrane. Although this hypothesis is not universally accepted, the evidence adduced by Professor Eccles in its support is certainly formidable. At all events, it would appear that a judicious combination of electrophysiological and biochemical techniques has much to contribute to our understanding of nervous action.

Among the many topics discussed in this book, two stand out as of special interest to the psychologist and philosopher. First, the question of neural plasticity and its relation to learning and memory; and second, the neurophysiology of the cerebral cortex. As regards plasticity, Professor Eccles devotes a great deal more attention to this important subject than it has hitherto elicited from the neurophysiologist. His own work on the prolonged effects of repetitive stimulation of spinal nerve roots leads him to conclude that even the simplest reflex arc possesses plastic properties. Effects apparently due either to 'use' or 'disuse' can be convincingly demonstrated at the spinal reflex level and can be explained, in the author's view, only in terms of acquired changes in synaptic efficiency. This conception of plastic changes at the synapse is then adduced to account for certain phenomena of learning, in particular conditioned reflexes. Although

the explanation given is admittedly sketchy, it does at least have the merit of conforming to established principles in neurophysiology. In a field in which speculation has roamed unbridled for at least three centuries this is a notable achievement.

In his account of the cerebral cortex, Professor Eccles largely confines himself to the study of potential changes evoked either by direct stimulation or by volleys in afferent fibres to the cortex. The main outcome of this work, in his view, has been to show that cortical neurones do not appear to differ in any fundamental way from neurones situated elsewhere in the central nervous system, in particular the motoneurones of the spinal cord about which our knowledge is most complete. He also suggests, on good evidence, that the spontaneous cortical rhythms studied by the electroencephalographer are due not, as is often supposed, to an inherent neural rhythmicity but to impulses circulating in self-reexciting chains or lattices of cortical neurones. Although Professor Eccles rightly regards the physiology of the cortex as largely unexplored territory, he is guarded as to the wisdom of direct attack. " It may be doubted ", he concludes, " if much effective progress will be made until more is known about synaptic transmitter mechanisms in the much simpler regions of the central nervous system."

The final chapter is devoted to an attempt to link what is known about the physiology of the brain with the problem of mental activity. This is beyond doubt the least satisfactory chapter of the book. The brain, Professor Eccles believes, " . . . is the sort of machine a ' ghost ' could operate, if by ghost we mean in the first place an ' agent ' whose action has escaped detection even by the most delicate physical instruments ". This may be so, but it will be borne in mind that similar statements have been made in the past about practically every organ in the body, not excluding the spinal cord. Physiology has flourished by disregarding them.

O. L. ZANGWILL.

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IX.—NOTES

ERRATUM

On p. 492 of No. 256 (October, 1955) in an article "The Fundamental Ideas of Pansomatism," by T. Kotarbiński, the formula (ex X = Y(Y est X), Df.) should read (ex X = ∃Y(Y est X), Df.

PROVISIONAL PROGRAMME: ARISTOTELIAN SOCIETY AND MIND ASSOCIATION JOINT SESSION 1956. July 6th to 8th, Aberystwyth.

Friday Evening, 6th July, 1956. Chairman: Professor J. N. Findlay. Address: Professor R. I. Aaron.

Saturday Morning, 7th July, 1956. (*a*) "Art and Symbolism." Chairman: Mr. A. Quinton; (i) Mr. R. W. Hepburn; (ii) Miss Iris Murdoch. (*b*) "The Law of Excluded Middle." Chairman: Mr. P. A. Grice; (i) Mr. P. Geach; (ii) Mr. W. F. Bednarowski.

Saturday Evening, 7th July, 1956 (Plenary Session). "The Consequences of Action." Chairman: Mr. P. Nowell-Smith; (i) Professor A. N. Prior; (ii) Dr. D. D. Raphael.

Sunday Morning, 8th July, 1956. (*a*) "Self-knowledge." Chairman: Mr. M. B. Foster; (i) Professor J. R. Jones; (ii) Mr. T. R. Miles; (*b*) "Historical Explanation." Chairman: Mr. P. L. Gardiner; (i) Mr. E. A. Gellner; (ii) Mr. P. G. Lucas.

Sunday Afternoon, 8th July, 1956 (Plenary Session). "Dreams." Chairman: Mr. Stuart Hampshire; (i) Mr. L. E. Thomas; (ii) Mr. A. R. Manser.

BRITISH PSYCHO-ANALYTICAL SOCIETY

As part of the celebrations of the Centenary of Freud's birth on 6th May 1956, the British Psycho-Analytical Society is organising a series of six lectures under the general title of "Psycho-Analysis and Contemporary Thought". These lectures aim at showing how Freudian theories and discoveries can be applied to fields other than purely clinical. The understanding of the dynamics of the unconscious help to increase our insight into such problems as the sense of guilt, or the nature of artistic creation, or in the understanding of important background factors in industrial conflicts. Of interest, too, is the light that can be thrown on the workings of the philosopher's mind by psycho-analysis, and how mechanisms connected with early emotional life may influence the philosopher's approach to "Truth". There will also be two lectures on the growth of the child, and the nature of important relationships to its teacher.